PEARL

The Journal of HEREDITY

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(SEE PAGE 30)

More Hybrids Before Koelreuter *The History of Eugenic Sterilization*

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The American Genetic Association

An incorporated organization devoted to promoting a knowledge of the laws of heredity and their application to the improvement of plants, animals, and human racial stocks

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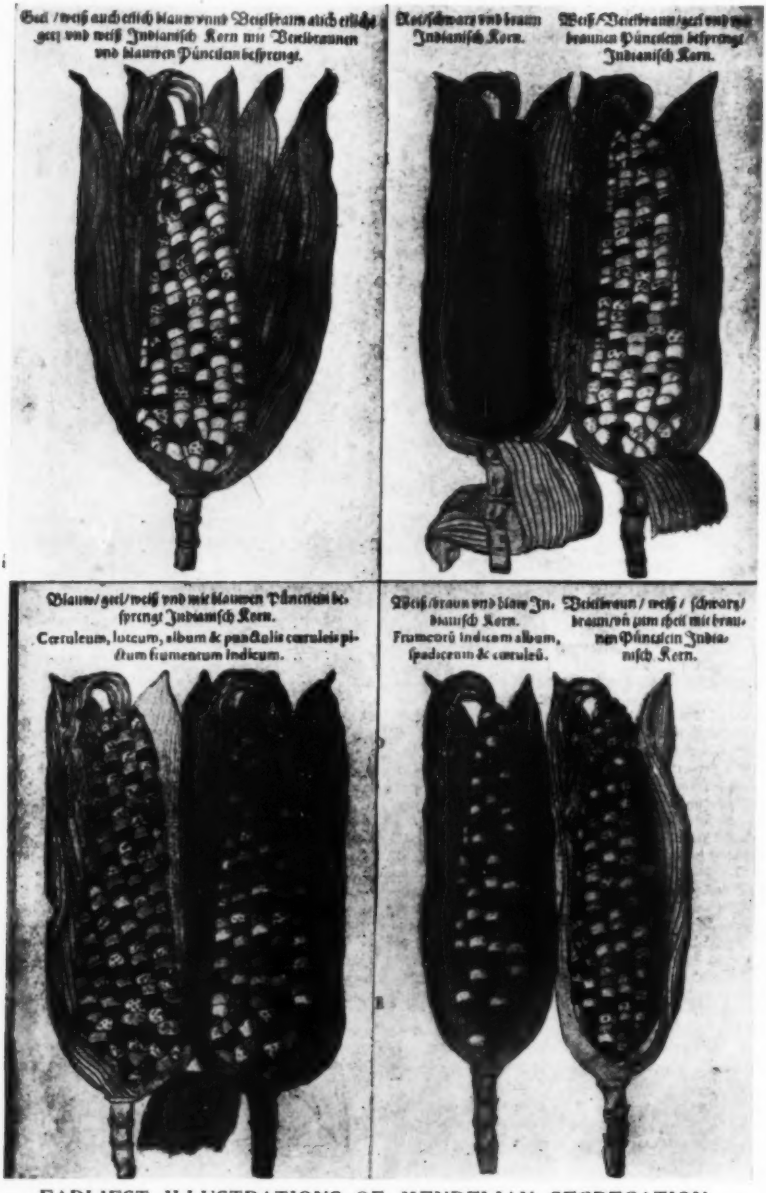
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Frumentũ indicum album, spadiceum & cæruleũ.
Beetbraun / weiß / schwartz/ braun/vñ zum theil mit braunen Pünctlein Indianisch Kern.

EARLIEST ILLUSTRATIONS OF MENDELIAN SEGREGATION

Frontispiece

Illustrations from Tabernaemontanus' *Krauterbuch*, showing ears of maize with grains of various colors,—white, blue, brown, black, and yellow. This book was originally published in 1588 (this edition 1613), but the illustrations were not colored until about two centuries later. The coloring follows the printed directions faithfully. These early observers of the effect of hybridization offered many fantastic explanations of the phenomena they reported. Not until a century later did Cotton Mather (whose theological eminence has distracted attention from his botanical attainments) offer the correct explanation for the occurrence of many colors on a single ear of maize.

MORE RECORDS OF PLANT HYBRIDIZATION BEFORE KOELREUTER

CONWAY ZIRKLE
*University of Pennsylvania**

IN a recent issue of this JOURNAL, the writer[30] described some early records of plant hybridization which had escaped the attention of the historians of botany. Since the publication of this paper, several unpublished documents have been found which not only clarify and supplement some of our existing records, but also bring to light the work of new investigators whose really important contributions have been overlooked. Indeed, from the very beginning, the development of genetics has been retarded by the failure of biologists to recognize the significance of hybridization. We can safely estimate that thirty years were lost by their neglect of Mendel. But Mendel was not the first to be ignored. One hundred and forty years before he published, another Cleric, an Englishman, casually observed the crossing of "pease," and noted the occurrence of different kinds in the same pod. Cromwell Mortimer,[22] in perhaps the only published reference to this work, deplored the fact that these hybrid peas were not planted and their progeny studied.

There is little doubt that Mendelian ratios were first observed in *Zea Mays*, and it is equally certain that the first botanists to record the presence of two or more types of grain on the same ear did not understand the cause of the mixture. We have it on the authority of Paul Dudley that as late as the first quarter of the eighteenth century both the Indians and the white settlers supposed the mixing of colors to be caused by the rootlets of different color varieties fusing underground. Two hundred years earlier, when the Europeans first became familiar with Indian corn, they did not consider variegated ears the result of a mixing of varieties but rather a *tour de force* on the part of the Deity whose real purpose was to astonish the inquisitive botanists. Many descriptions of this spontaneous hybridization appeared during the next century and one even as late as the year preceding Cotton Mather's correct interpretation of the phenomenon.

No adequate history of early plant hybridization has ever been written and almost nothing is known of such work preceding Koelreuter's investigations. Many essential documents remain unpublished and many of the earlier published records are not available to modern investigators. Some of these forgotten records were printed in the paper referred to above.[30] Those presented here include, among others, some earlier descriptions of hybrid varieties in *Zea Mays* even when the writers themselves did not know what they were describing. While these passages may have some interest for the geneticists who are at present investigating corn, their reprinting is really justified in that they form a necessary background for our evaluation of the first correct interpretations of hybridization. The first botanists who described understandingly the crossing of different varieties and species were extremely casual and often inaccurate in their observations, yet they did accumulate a mass of verifiable knowledge, and as a result hybridity was well understood when Koelreuter undertook his classical experiments.

Records of Different Types of Grain on the Same Ear of Zea Mays

1. Jakob Theodor of Berg-zabern (Tabernaemontanus) published his *Krauterbuch* in 1588. The following

*This investigation was aided by a University of Pennsylvania Faculty Research Grant.

passage is taken from part I, page 640, under the heading *Von dem Indianischen Korn:*

And one sees an especially great and wonderful mystery in these spikes. "Gott der HErr" through the medium of nature which must serve everyone disports himself and performs wonders in his works and so notable in the case of this plant that we must rightly be amazed and should learn to know the One True Eternal God even from his creatures alone. For, some spikes of this plant, together with their fruit, are quite white, some brown-black and dazzlingly beautiful, some yellow, some brown, and the others white, brown and blue intermixed. Thus, sometimes some rows are half white, a second series brown, and the third blue; and some grains, accordingly, are mixed with each other and transposed. Again, sometimes one, two, or three rows are white, the next rows blue, then again white and after that chestnut-brown; that is, they are interchanged on one row and run straight through on another. Some spikes and their grains are entirely yellow, others entirely brown, some are white, brown and blue, others violet, white, black and brown; of these, the white and blue are prettily sprinkled with small brown dots, as if they had been artistically colored in this way by a painter. Some are red, black and brown, with sometimes one color next to the other, while at other times two, three, even four colors, more or less, are found one next to another in this way. Again, some are white, violet and yellow, and of these the white and yellow are colored with pretty, small brown dots. Again, one finds that the fruit of some spikes is yellow and white; some also are blue and violet, others are yellow and white. Of these, the yellow and white are sprinkled with violet and blue dots, and, on the other hand, the violet and blue spikes with yellow and white dots. Some spikes, again, are red and chestnut-brown, with here and there one color placed next to the other. Others are golden yellow and white, also with one color placed next to the other. Some are blue, yellow and white, of which the white and yellow are sprinkled with blue dots. So that, as we have said before, one must wonder greatly at this fruit. And, (all told), there are fifteen kinds of this Indian corn when all the fruit has colors variegated in this way.

I have seen still another kind of this corn at the house of Herr Stefan Vein, steward of the Elector's Imperial Palace at Libenau, who had it in his garden; **** The fruit has variegated colors, as white, red, chestnut-brown, yellow and brown-black, at times with the colors intermixed or transposed; in fact, just as we have indicated them in connection with Indian corn. I should like to have had this plant plucked and set before the eyes of the gracious reader, but at the time I saw this growth I had no painter at my disposal. I shall make amends, however, in time, should "GOtt der HERR" please to continue to grant me this temporal life, which is sealed in his hand and plan.

The frontispiece is from the edition of 1613. Of course the original figures were not colored when the book was issued but were painted in much later. The copy from which these illustrations are taken was decorated by Johan Ludwig Müller, born in 1747 at Nürnberg, Germany, died in 1822 at York, Pennsylvania. He settled in America in 1771 and probably colored the illustrations of the American Plants after he arrived in his new home.

2. P. A. Matthiola's *Neu Vollkommenes Krauter-Buch* was published in Basel in 1590 and went through many editions. The following quotation is from the revised edition of 1678:

INDIAN CORN

They are seperated into four varieties. One with brown, the other with light red, the third with yellow and the fourth with white grains; also another is found containing red, blue and yellow grains mixed, which appears very amusing.

3. John Gerard published his *History of Plants* in 1597. His illustrations were in part taken from Tabernaemontanus. The following passage is on page 74, under the heading *Of Turkie corne.*

OF TURKIE CORNE.

Of Turkie corne there be divers sorts notwithstanding of one stocke or kinred, consisting of sundrie coloured graines, wherein the difference is easie to be discerned; and for the better explanation of the same I have set forth to your view certaine eares of different colours, in their full and perfect ripeness, and such as they show themselves to be, when their skin or filme doth open itself in the time of gathering.

An illustration is labeled:

"Blew and White Turkie Wheate mixed"

4. The New England settlers had not been long in the New World before they were forced by circumstances to take an intense interest in any possible source of food. The following passage is taken from *Purchas, His Pilgrims*. Ex. Ser. Vol. 32, p. 317.

"A Relation or Journall of a Plantation setted at Plimoth in New England, and proceedings thereof: Printed 1622 and here abbreviated."

Wed. Nov. 15, 1620.

We went on further and found new stubble of which they had gotten Corne this yeare, and many Walnut trees full of Nuts, and great store of Strawberries, and some Vines; passing thus a field or two, which were not great, we came to another, which had also bin new gotten, and there wee found where an house had beene, and foure or five old Plankes laied together; also wee found a great Kettle, which had beene some Ships kettle and brought out of Europe; there was also an heape of sand, made like the former, but it was newly done, wee might see how they had padled it with their hands, and found a fine great new Basket full of very faire Corne, some yellow, and some red and others mixt with blew, which was a very goodly sight: the Basket was round, and narrow at the top, it held about three or four bushels, which was as much as two of us could lift up from the ground.

5. Kaspar Bauhin published the *Theatri Botanici* in 1620. The following passage is taken from the second edition of 1671, page 24, under the heading *frumentum indicum*.

Since this corn varies in the color of the spikes, Tabernaemontanus represents (pictorially) sixteen varieties of spike and Gerard twelve. These are of both pure and mixed colors. The pure are white, yellow, golden yellow, violet, chestnut-brown, and black. The mixed colors appear as follows: White and brown; then with blue, blue, white and black partially mixed with brown dots; white, violet, yellow with brown dots; red, black, and brown; yellow and white, sometimes blue and violet, sometimes yellow and white, sprinkled with violet and blue dots; red and chestnut-brown, golden yellow and white; blue, yellow, white, sprinkled with blue dots; and, finally, blue, yellow, violet, and white.

6. Joannes de Laet published *Novum Orbis, seu Descriptionis Indiae occidentalis* in 1633. The following is from page 7.

Indian maize, which the inhabitants of Virginia call *Pagatowr*, grows in great abundance here and yields grain at times red in color, at times yellow, and at times of mixed colors choicely variegated. It rises to a height of six or seven, sometimes even ten feet, and bears three or four ears, occasionally, however only one. These are loaded with five or six hundred grains, and, when the crop is especially plentiful, even seven hundred; the Indian corn being singular for its luxuriant foliage.

7. John Winthrop, F.R.S., was a Governor of Massachusetts. He wrote a paper entitled *On the Culture and Use of Maize* which he published in 1678 in the Philosophical Transactions. The quotation is from page 1065.

The Ear is for the most part, about a span long, composed of several, commonly 8 rows of Grains, or more, according to the goodness of the ground; and in each row usually above 30 grains. Of various colours, as Red, White, Yellow, Blew, Olive, Greenish, Black, speckled striped, &c, sometimes in the same field, and the same Ear. But the White and Yellow are the most common * * * *

In the more Northerly parts, they have a peculiar kind called *Mohawks* Corn, which though planted in *June*, will be ripe in season. The stalks of this kind are shorter, and the Ears grow nearer the bottom of the stalk, and are generally of diverse colours

8. Robert Beverly published a *History of Virginia* in 1705. He noted not only the mixture of colors but also of "flint" with "dent."

The late ripe corn is diversified by the shape of the grain only, without any respect to the accidental differences in color, some being blue, some red, some yellow, some white and some streaked. That therefore which makes the distinction, is the plumpness or shriveling of the grain; the one looks as smooth and as full as the early ripe corn; the other has a larger grain and looks shriveled, with a dent on the back of the grain, as if it had never come to perfection; this they call she corn. This is esteemed by the planters as the best for increase, and is universally chosen by them for planting; yet I can't see but that this also produces the flint corn, accidentally among the other.

9. William Salmon's *Herball* appeared in 1710. From page 1253—

OF WHEAT INDIAN OR MAIZE

II. *The Kinds.* We have but One Species thereof, but some may account them Two, by reason of their *Magnitude*, which I take to be rather from the Nature of the Soil: a rich Soil affording a very large sort: whereas a poor Soil gives you only a Dwarf kind of Plant: But there is a great Variety in the Colors of the Ears, some being all White, some all Yellow, some all Red and some Blew. And again, some Ears have grains of all those Colors at once; but this difference, we account makes no differing Species of the Plant.

10. Just one year before Cotton Mather explained the mixing of different colored grains on the same ear of corn, the posthumous edition of Robert Morison's *Plantarum historiae universalis oxonensis appeared* (1715). From Vol. II, p. 248.

This corn varies in the color of the spikes and grains. The colors are either pure or mixed. The pure are these: white, yellow and golden yellow, violet, chestnut-brown, red, and black. Mixed colors appear in the same spike and sometimes in the same grain. The combinations are: white-brown, and blue, white, black partially mixed with brown dots; white, violet, yellow, with brown dots; red, black and brown; yellow, white, sometimes blue and violet, sometimes yellow and white, sprinkled with violet and blue dots; red and chestnut-brown, golden yellow and white, blue yellow, and white, sprinkled with red dots; and, finally, blue, yellow, violet and white.

Cotton Mather

On September 24, 1716, Cotton Mather wrote to James Petiver, F.R.S. The letter was not published in the Philosophical Transactions but passed into the collection of Sir Hans Sloane and thus into the British Museum. Kittridge called attention to it.[12,13] The passage in this letter which describes hybridization in *Zea Mays* and *Cucurbita Pepo* was published by Cotton Mather in *Religio Philosophica* or *The Christian Philosopher*, London, 1721, with some slight changes in the wording. Murdock[23] reprinted this version, and it was included by the writer in the paper cited previously.[30] The original letter has probably escaped publication and, as it contains the earliest account of plant hybridization yet found, it is here printed in full.

Boston, N. England.
24^{d} VII^{m} 1716.

S^{r},

Tis high time for me to make you some return, that may express my sense of the Obligations, which your Letters, with what accompanied them, have laid upon me.

Tis a Vast Load, and a somewhat uncommon Variety of Employments, which my Various Capacities at home and Correspondences abroad, impose upon me,—Whereof my Friends are so sensible, that their Candor forever has a pardon ready for me, when at any time I fail of keeping Some, in my compliance with their Expressions.

But I hope now to make some Reparation for my Delay of my Duty, and Endeavour an Entertainment that may find some Acceptance with you. I perceive that Botanic studies are those to which you have more singularly applied your Inquisitive and Ingenious Mind. And now that I may find something which may be a little Relishable for you, after that I have mentioned one

or two curious Observations upon some occurences in the Vegetable Kingdome, I will fulfil a promise which I made unto my most valuable Dr. *Woodward*, a few Weeks ago, concerning certain *American Plants** which I was collecting for you.

In a Field not far from the City of *Boston*, there were lately made these Two Experiments.

First: my Friend planted a Row of *Indian Corn* that was Coloured Red and Blue; the rest of the Field being planted with corn of the yellow, which is the most usual colour. To the Windward side, this Red and Blue Row, so infected Three or Four whole Rows, as to communicate the same Colour unto them; and part of ye Fifth, and some of ye Sixth. But to the Leeward Side, no less than Seven or Eight Rows, had ye same Colour communicated unto them; and some small Impressions were made on those that were yet further off.

Secondly: The same Friend had his Garden ever now and then Robbed of the *Squashes*, which were growing there. To inflict a pretty little punishment on the Theeves, he planted some *Guords* among the Squashes, (which are in Aspect very like 'em) at certain places which he distinguished with a private mark, that he might not be himself imposed upon. By this method, ye Thieves were deceived, & discovered, & ridiculed. But yet the honest man saved himself no Squashes by ye Trick; *for* they were so infected and Embittered by the Guords, that there was no eating of them.

Several Useful Hints relating to Vegetation and Agriculture, may be taken from these Experiments; which I wholly Leave to your Sagacity, and that of ye philosophers to whom you may see cause to mention them.

What remains is, To make you a Tender of Six or Seven plants, Which are here esteemed peculiar to *America*. I have not yet found them in any *European* Herbals; and if you find that I have been mistaken, you will pardon my Ignorance, and will accept my Intention. I have preserved the plants, as well as I can in a Book of brown paper; and have presumed so far as to Impose Names upon them; and have added Schedules, declaring the vertues and Uses for which ye Indians have employ'd them, and in which ye English have been also confirmed by their Experience.

I have committed the Book unto the care of an honourable Friend, Mr. *Edward Loyd*, (one of ye Judges,) who will do me the honour to deliver it with his own hand.

This is but a part of the Collection that I intend for you; because this year I unhappily miss'd the Seas(on) of securing several plants unknown to ye, *European(s)* which, if I live to another Season, I will do my best that I may be a Master of them.

In the mean time, I shall not be altogether wanting in my Essayes to yeeld all possible Obedience unto your Commands. And I hope annually to treat the Royal Society also; with such a Number of Communications, that if every Member of that Illustrious Body whose Name stands in ye Catalogue (an Honour not yet obtained for mine†) will do but half as much, the stores in your Collection will soon become Considerable.

That the Blessing of Heaven may attend y(our) person, and all your generous Endeavours w( ) in the world; is the hearty prayer of,

Sir,
Your most affectionate
Friend & Serv[t].
COTTON MATHER.

Mr. Pettiver

Thomas Fairchild

Thomas Fairchild is credited with having made the first artificial plant hybrid. Unfortunately no account of this work, written by Fairchild himself, has yet been found, and we have to

*From Cotton Mather's Diary of 1716. Sept. 12, G.D. What shall I do to render my Kinsman at *Newton*, considerably useful? Employ my Kinsman at *Roxbury* to make a Collection of *Plants*, peculiarly *American*.[30]

†Cotton Mather was elected to the Royal Society on July 27, 1713, but, living in America, he could not be personally inducted into membership. The matter was not finally settled until April 11, 1723.[32]

Boston, N. England
24. Jun. 1716.
Rec. 8 Jan. 1716/7

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(To) Right Honble for me, to make your
Grace Known, that my Dispatch my Letter of the 8th Jan.
ances, which your Grace, with whom accompanied that
Could Could reform me.

To a Vast Load, and a Somewhat Circumstantial Relation
of Transactions, which my Various Employments in Civil
and Commandments abroad, imposed upon me, I have
my Inclination and so Capable, that I shall Candor Grace
Shall a Pardon ready for me if I Relate as my Mind I shall
of Reading Grace, in my Compliance with that Request

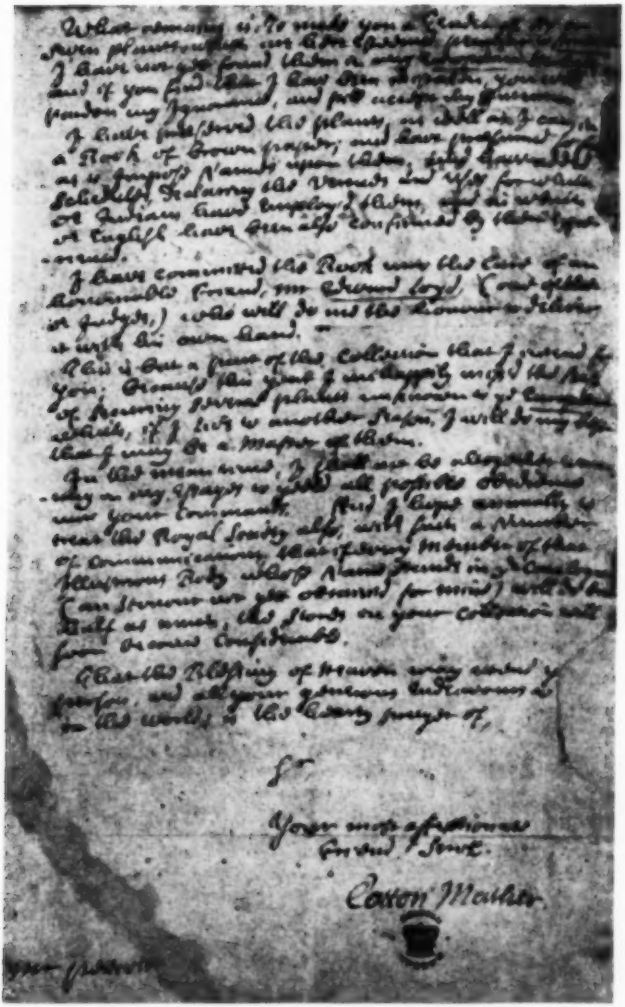
But I hope now to make some Reparation for my
Delay of my Duty, and Endeavour an Excuse that
that may find some Acceptance with you.

I presume that Governor Dudley has Long ere
you have more Singularly applied your Duty to
Righteous Hand. But now that I have find some
which may be a Little Satisfaction for you, after that
I have witnessed and in my Several Offices
upon Some Occasions in the Virginia Regt
I shall fulfil a promise while I made unto my
valuable Dr Woodward, a few Months ago
Curio in Bottom plants, which I was
you.

For a Full and Just Account this City of Boston, I
who Lately made Such Observations,

First, my Second Volume a Row of Gardens
Line was Coloured Red and Blue, that with Apples
being planted with One of the Colour
usual, Colour. So the Windward Side Red and
Blue Row, so Inverted Plants or Some which
a Communicate the Second Colour, Many
Some of the Fruit, and Some of the Leaves. Red
Wind Side, on the Blue Ribbon or Light Purple
the Second Colour, Communicated more Plants, and
Second Growth was more on that Plant
Similar off.

Secondly, Cabbages Coloured Red the Garden
I then Ribbed of the Apple Tree, which
Plant. So that Cabbage Leaves Coloured
Cabbage the Plants Second Growth
Cabbage are of Apple Tree, but I am
within the Apple Tree, with a Strong Smell
Red might Second Growth upon
Meadow, Red Plants, Purple
Leaves, Blue Plants, Purple
Apple Tree, Second Growth
Leaves, Blue Plants, Purple
Apple Tree, Second Growth
Leaves, Blue Plants, Purple
Apple Tree, Second Growth



THE EARLIEST ACCOUNT OF PLANT HYBRIDIZATION

Figures 1 and 2

Under date of September 24, 1716, Cotton Mather wrote to James Petiver describing (paragraph 6) the "infection" of a yellow-seeded variety of maize by one having red and blue seeds. While Mather in this letter does not mention the infecting agent, his reference to the windward and leeward location of the rows leaves little doubt that he considered the wind-blown pollen to be the cause of this phenomenon. In 1721 Mather specifically mentioned the "male sperm" which "fructifies the seed."

depend on the records of his contemporaries for our knowledge of his hybridization of Dianthus. A number of these records have been collected recently by the writer.[30] The fact that Fairchild's hybrid was artificially produced and not a mere accidental natural cross is inferred from Bradley's (1717) account:

* * * * * the Farina of one will impregnate the other and the *Seed* so enliven'd will produce a *Plant* different from either, as may now be seen in the garden of *Mr. Thomas Fairchild* of Hoxton, a Plant neither *Sweet William* nor *Carnation*, but resembling both equally; which was raised from the *Seed* of a *Carnation* that had been impregnated by the *Farina* of the *Sweet William*.

The following extracts from the Minutes of the Royal Society give an actual account of the origin of the Dianthus hybrid.

Extracts from the *Journal Book* of the Royal Society Vol. XII, 1714-1720. p. 411

February 4th, 1719/20.

Sr. Hans Sloane, Vice-President in the Chair.

The minutes being read Mr. Fairchild, Gardener at Hoxton had leave to be present.

Dr. Blair communicated an account of several Botanick Experiments made by Mr. Fairchild and others, which serve to confirm the truth of the Drs. Notions about the different sexes in Plants & the manner of the circulation of the Sap as they are explained in his late Book of Botany.

* * * * * *

p. 412

The other Experiment was made by Mr. Fairchild some years ago. He found a plant in his garden of a middle nature between a sweet william & carnation July flower (a specimen of which was produced before the Society) it grew in a bed where the seed of each of those flowers had by accident been thrown promiscuously, & he takes it to be an heterogeneous production from those two different flowers. * * * * these new sort of plants produce no seed, but are barren like the Mule or other mungrel animals which are generated from different species.

If this account given by Blair is accurate, and we have no reason to doubt it, for Fairchild himself was present when it was delivered, it would seem that Fairchild was at least as casual with his famous "Mule Plant" as any of the other eighteenth century hybridizers. On the other hand Bradley definitely states that the carnation was the maternal parent and that the sweet william was the paternal, facts which could not be learned from the incident as described by Blair. It is true that Bradley had few scruples and was dishonest on several occasions, yet we cannot for this reason entirely discount his testimony. He apparently had nothing to gain by a false description of Fairchild's experiment.

Fairchild himself was a first class experimenter who suffered from the fact that most of his results were recorded by his doctrinaire friends. For him to find a natural Dianthus hybrid and experiment no further would be inconsistent with what little we know of his character. Indeed, as late as 1722, he stated in *The City Gardener*[9] that he was continuing his researches upon "the generation of plants," a phrase which, at this time, often referred to hybridization.

We can best reconcile our fragmentary records by assuming that the Dianthus hybrid first appeared spontaneously and that later Fairchild secured an artificial hybrid by crossing a female carnation with a male sweet william.

Richard Bradley

Richard Bradley is credited with contributing to our knowledge of sex in plants by noting that castrated tulips bore no seed. In the same chapter of his book, *New Improvements of Planting and Gardening*,[3] in which he recorded this observation, he described Fairchild's hybridization of Dianthus. His own discussions of hybridizaton, however, have generally been overlooked, although his observations of variety crosses in Primula (Auricula) have been extensively quoted by Rob-

erts.[26] The following two passages describe this work.

Pt. I, pp. 22-23.

'Tis from this accidental Coupling that proceeds the Numberless Varieties of *Fruits* and *Flowers* which are rais'd every Day from *Seed*. The yellow and black *Auriculas*, which were the first we had in *England*, coupling with one another, produced *Seed* which gave us other Varieties, which again mixing their Qualities in like manner, has afforded us by little and little, the numberless Variations which we see at this Day in every curious *Flower-Garden*; for I have saved the *Seeds* of near a hundred plain *Auriculas*, whose *Flowers* were of one Colour, and stood remote from others, and that *Seed* I remember to have produced no Variety; but on the other hand, where I have saved the *Seed* of such plain *Auriculas*, as we have stood together, and were differing in their Colours, that *Seed* has furnish'd me with great Varieties, different from the *Mother Plants*.

Pt. II, pp. 100-101.

WITH these Perfections we may account an *Auricula* to be good, and from such only we ought to save Seeds for sowing and propagating others, if we hope for Success; and as a help to our design let us consult the Chapter relating to the Generation of *Plants*, and improve the Variety by placing such Flowers as are of the most different Colours together whilst they are in *blossom*; that so the *Seed-Vessels* of the one may receive the *Dust* of the other, and by that means give us an agreeable Mixture of Colours in those *Plants* we raise from Seed; and it would be well worth our Enquiry, whether the Seeds thus Impregnated partake more of the Shape or of the Colours of the *Flowers* of the Mother *Plant*; to be certain of which, the *Plant* you make Tryal of must be castrated of its *Apices*, before they are ripe or burst open.

Bradley's account of hybridization in Auricula is probably correct. He was not so fortunate, however, in his description of the effect of foreign pollen upon fruits and melons. While it may be true that foreign pollen affects the parthenocarpic fruit of self sterile varieites, Bradley, in the following passages, certainly carried Metaxenia too far.

Pt. I, pp 24-25.

We may learn from hence, that the *Fruit* of any *Tree* may be adulterated as well by the *Farina* of one of the same Sort, which perhaps may be sickly and of a Dwarf Kind, as by the *Dust* of some other Kind near a-kin to it, and worse than itself. Now as such Couplings may be very frequent in common *Woods*, so would I recommend the Choice of *Seed*, to be made only from such *Plants*, or *Timber*-Trees as excel in Greatness or other good Qualities, and are far distant from others of meaner Sorts, which might degenerate their *Seeds*, and cross our Expectations when they come to grow up; and this is as necessary to be observed among *Vegetables*, to maintain their good Qualities in the young *Plants* they are to produce, as it is in the Breeding of *Game-Cocks*, *Spaniels*, or *Running-Horses*.

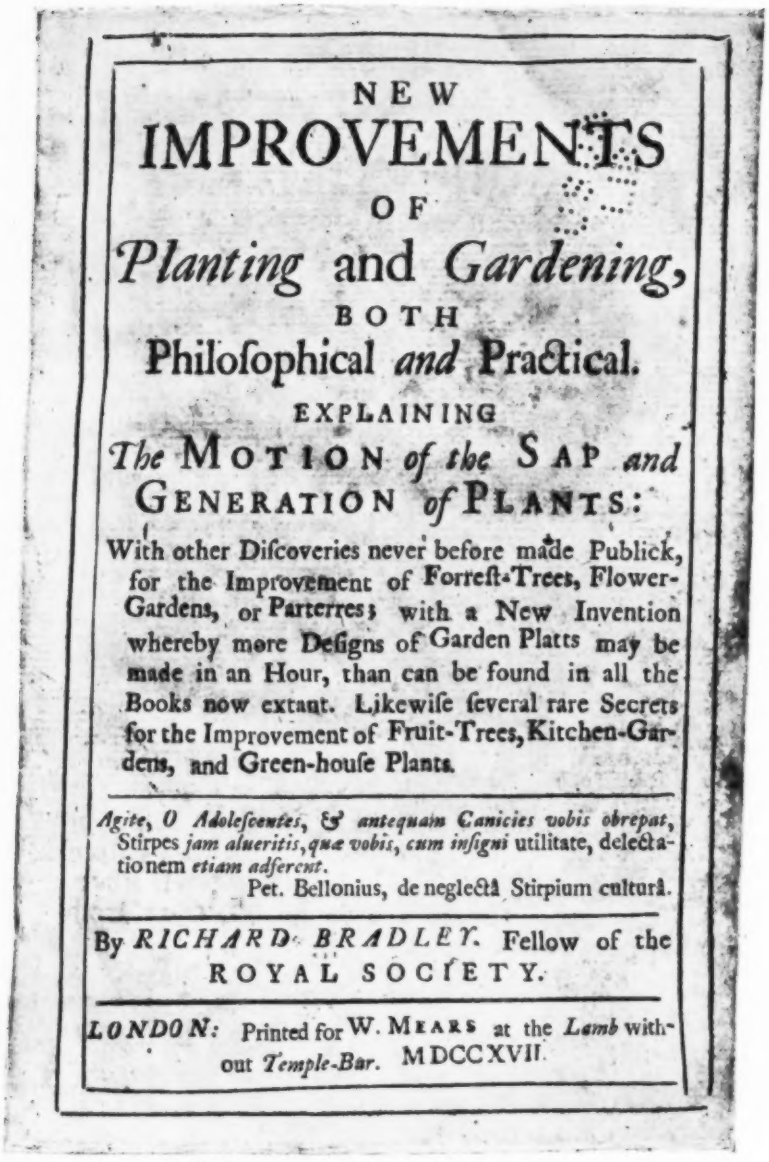
There is but one sort of *Plant* that I know of, which seems to be out of this Danger of coupling with other Sorts, and consequently of either improving or diminishing the Qualities of its *Seeds*, and that is the *Mistletoe*; the parts of its Flowers are indeed as apt to Generation as those of other *Plants*, but I have never seen any Variety of this *Plant*, or do I know of any other nearly enough related to it to *engender* with it; * * * * *

Pt. I, p. 22.

By this Knowledge we may alter the Property and Taste of any *Fruit*, by impregnating the one with the *Farina* of another of the same Class; as for Example, a *Codlin* with a *Pairmain*, which will occasion the *Codlin* so impregnated to last a longer Time than usual, and be of a sharper taste; or if the *Winter Fruits* should be fecundated with the *Dust* of the *Summer Kinds*, they will decay before their usual Time; and it is from this accidental Coupling of the *Farina* of one with the other, that in an Orchard where there is Variety of Apples, even the Fruit gather'd from the same *Tree*, differ in their Flavour and Times of ripening, and moreover the *Seeds* of those *Apples* so generated, being changed by that Means from their Natural Qualities, will produce different kind of *Fruits* if they are sown.

Pt. III, p. 114.

For the more certain producing *Melons* of a right Flavour, let me advise that no *Cu-*



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Agite, O Adoleſcentes, & antequam Canicies vobis obrepat, Stirpes *jam alueritis, quæ vobis, cum inſigni* utilitate, delectatione *etiam adferent.*

Pet. Bellonius, de neglectâ Stirpium curturâ.

By *RICHARD BRADLEY.* Fellow of the ROYAL SOCIETY.

LONDON: Printed for W. MEARS at the *Lamb* without *Temple-Bar.* MDCCXVII.

TITLE-PAGE OF BRADLEY'S BOOK

Figure 3

Richard Bradley (16??—1732) noted in the first edition of this book (1717) the fact that castrated tulips produce no seed, and described the technique of hybridization of primroses. Here is also to be found one of the contemporary descriptions of the experiments of Thomas Fairchild, who is credited with making the first artificial plant hybrid,—between the Pink and the Sweet William.

cumber Plants be set near them, lest the *Male Dust* of the *Cucumbers* should happen to be carry'd with the *Wind* upon the *Blossoms* of the *Melons*, and perhaps set them for Fruit, which will then certainly give the *Melons*, so produced, the relish of the *Cucumber* in Proportion as the *Farina* happens to fall in greater or lesser Quantity.

These passages were known to Charles Darwin and were cited as evidence that the male elements influenced the mother's body.[8] Unfortunately, Darwin, at that time, was marshalling all such instances which he could collect in an attempt to prove telegony.

Bradley was also interested in hybridizing Wall Flowers, as is shown by the following:
Pt. II, p. 91.

The Seed-Vessels of this and other single Kinds of it, as well as their Flowers, are so like those of the *Stock July-Flowers*, that I am of Opinion they might be made to impregnate each other's Seeds if they were planted nigh enough together, and from such Coupling, perhaps, might be produced a *Stock July-Flower* with Yellow Blossoms. It is what I design to try, my self, as well as many other Couplings of the like Nature; and altho' the Seed of such a neutral *Plant* would not be made to grow, the Species may be continued and increas'd by planting *Slips* or *Cuttings* of it, as is common in the Culture of *Double-Stocks*.

Thomas Knowlton

Thomas Knowlton was born in 1692. As a young man he was a gardener in the service of William Sherard. On the death of Sherard in 1728 he entered the service of the Earl of Burlington and moved to Lanesborough in Yorkshire. He died there in 1782. He was a friend of a number of the best botanists of his day, among others Sir Hans Sloane, Patrick Blair, Mark Catesby, and, of course, William Sherard. Knowlton is known as the discoverer of the Moore Balls or Globe Conferva (*Conferva Aegagropila Lin.*). He also had several of his letters published in the Philosophical Transactions.[14,15,16,17] In these communications he described the discovery of the ruins of the ancient Roman town, Delgovica, and the finding of horns of an extinct species of deer. A genus of the Ranunculaceae is named for him.

Knowlton's observations on hybridization and sex in plants were reported by Patrick Blair to the Royal Society in 1720, at the same meeting in which Fairchild's Mule Plant was exhibited. The following extract is taken from the *Journal Book* of the Royal Society, Vol. XII, pp. 411-412.

Mr. Knowlton Gardener at Offly place in Hertfordshire took two parcels of Turkey wheate. He putt the grains of one of these parcels into the ground singly grain by grain, the other parcel he sew'd in Drils promiscuously after the usual manner.

That which was sown singly shed its dust before the female embrij began to appear & most of it miscarried. The few seed which ripend, he committed to the ground in September but it produced nothing.

The other parcel of wheat ripened very well & produced plentifuliy 3 or 4 ears each Stalk with 10 or 11 Rows of Seed in each ear. This confirms what is laid down in the 238 & 239 page of his (Blair's) Book of Botany. That the union of male & female flowers are necessary to fructification.

In regard to the crossing of species Blair cites the Dianthus hybridization of Fairchild already quoted above and then adds:

The like has been observed by Mr. Knowlton who found a new plant partaking of the natures of a China pink & sweet william. And they both observe that these new sort of plants produce no seed, but are barren like the Mule or other Mungrel animals which are generated from different species.

Thomas Henchman

In the Autumn of 1729, the Reverend Mr. Henchman, Prebendary of Salisbury,* noted with surprise the

*I am indebted to Bishop John Randolph of Salisbury for the following information: The Reverend Thomas Henchman was attached to Salisbury Cathedral as Prebendary of Gillingham Miner from 1717 until his death in 1746. He was buried in the Cathedral.

spontaneous crossing of two varieties of "Pease" which had been planted side by side the previous Spring. He was especially interested in the occurrence of different colored peas in the same pod. On June 3, 1731, he wrote an account of his observations and sent it to Sir Joseph Ayloffe, who read it at a meeting of the Royal Society on July 1, 1731. The paper was not published in the *Philosophical Transactions* and seems to have attracted no attention until Mr. Cromwell Mortimer,[22] Secretary of the Society, described it briefly in a foot note in the volume of the Transactions issued in 1745. The foot note received little attention, although Charles Darwin knew of it and referred to it briefly.[8] The actual presentation of the paper is described in the *Journal Book* of the Royal Society. From Vol. 13, p. 635 (meeting July 1, 1731):

Sir Joseph Ayloffe shewed the Society two or three peas pods containing Heterogeneous Seeds each pod having peas some white and some blew, and he communicated an account of their manner of production as he received it from the Revd. Mr. Henchman Prebendary of Salisbury in a paper dated 3d. June last. Who says that in the Spring 1729, a piece of his Ground being plowed up to be sowed with white peas and proving to be more than enough for the seed Designed for it; One part of the remainder consisting of two double rows, was sowed with blew peas, and the other part being about four feet in width was left unsowed for a walk between the two plots of sowed ground. The event was that in flowering they mutually Impregnated each other so as to Generate white and blew peas within the same pod, sometimes more of one and sometimes more of the other according to the Specimens produced before the Society.

Sir Joseph had the thanks of the Society for his communication.

The Register Book of the Royal Society contains Mr. Henchman's letter (Vol. 16, p. 8).

Of the mixture of blew and white Peas in one Pod: communicated in a Letter to Joseph Ayloffe, Esq. by the Revd. Mr. Henchman Prebendary of Sarum: dated Sarum June 3rd. 1731.

In the Spring, 1729, I sowed a Piece of Ground for my Garden with white Peas; and my seed not holding out to sow all the Ground dug up, I sowed two double Rows of the Peas, with an alley of four Feet wide between. In the Autumn, walking one evening between the Rows, I gather'd some for seed; and one of the shells opening in the gathering I was surpriz'd to see one Blew Pea at the End next the Stalk, with six white Peas. I laid it carefully by, looking on it as strong Proof of Mr. Bradley's hypothesis of the Generation of Plants, wch I was much pleased with from the first Reading of it, as very ingenious and highly probable, as I thought. But my curiosity did not lead me to enquire at that Time whether there were any more Shells with such a Mixture of the two Sorts of Peas in them.

In the Spring, 1730, as my Boy was rubbing out the Seed saved from that Plot to sow again, I observ'd a great many blew Peas among the White. Upon which I stop'd him, and took the Remainder, which I shell'd carefully my self one by one; and found great Variety of Intermixture of the white and blew Peas in the same shell: sometimes one white (or blew) only at one End, as in the first I took Notice of, sometimes at each end; sometimes two white (or blew) with one of the other colour interchangeably. I then laid by at least fifty Shells (several of each different Mixture) which I thought would be enough to satisfy the Curiosity of all my Friends, and fully establish Mr. Bradleys hypothesis. I kept them all the last Year, and this Spring gave several shells of them (of different mixtures) to Mr. James harris Junior to carry up to London; and after that neglected the rest. I have still a few shells left; but they having lain unregarded in a window, the Sun hath scorch'd and forced them open.

Not having Plots of white and blew Peas standing near one another the last Year, I

have not found any such mixture in the several Parcels of Seed then saved. In the beginning of this Spring I sow'd a Plot on purpose with a double Row of blew Peas on each outside of the Piece and all the middle Rows white. But the Weather prov'd so unkind at their first coming up, that it kill'd almost all of them. Yet I have two double Rows left (in a poor Condition) which I shall take Care to preserve for Seed; to see whether it will prove so upon a second Experiment, which I think there is no great Reason to doubt of.

And so the experiment came to an end. We must agree, however, with Mr. Mortimer who ended his foot-note reference by the comment:

But it is a pity he did not pick out a sufficient Number of blew Pease from among the white, and sow them by themselves, in order to see what colour'd Peasc this mix't Breed would have produced.

Benj. Cooke

Mr. Benj. Cooke F.R.S. lived at Newport in the Isle of Wight. He was a cousin of Peter Collinson and, like Collinson, interested in things botanical. Some of his communications to Collinson were later sent to Sir Hans Sloane while others were published in the *Philosophical Transactions*. Cooke's observations of hybridization were enthusiastic rather than critical, yet he did observe the crossing of color varieties of *Zea Mays*. His description of the corn silks blushing as a result of their commerce with an alien pollen is fanciful to say the least, and we should perhaps interpret his hybrid apples as bud sports. On the other hand, he made real attempts to hybridize plants and he correctly interpreted the mixture of different colored grains in the ears of corn.

The following letters are reprinted from the *Philosophical Transactions*, Vol. 43, pp. 525-526:

Extract of a letter from Mr. Benj. Cooke, *F.R.S. to Mr.* Peter Collinson, *F.R.S. concerning the Effect which the* Farina *of the Blossoms of different Sorts of* Apple-trees *had on the Fruit of a neighboring Tree.*

Newport (Isle of Wight), *Oct.* 1745.

Dear Cousin,

I have sent you some *Russetings* changed by the Farina of a next-door Neighbour, whose Name I wanted Skill to know; but can only say, that the *Russeting* has exactly acquired his Face and Complexion.

(*Mr. Collinson* then produced several Samples of the Apples; an unteinted Russetting; a Russetting changed in Complexion, which grew among a great Cluster of unalter'd Brethren; and some Apples of the other Tree, which had caused the Change in the Russetings, and whose Fruit had in Return received a rough Coat from the Russettings.)

Theophrastus takes notice of this *Παραλλαγῆ*, as he calls it; and tells us the old Divines were wont to make a great pother about it, and foretel great Events by it: *Pliny* informs us, there was one who wrote a whole Book about such Changes. But the Use I should make of it, is chiefly this, that it may be of Importance to the Curious in Fruits, to take care how their Trees are sorted, and what Company they keep. For tho' this Change be not so conspicuous in Apples which have a smooth green Coat, as in the Russet-breed, yet one may suppose Impressions of this sort often made on them; and perhaps their Juices alter'd for the better or worse.

Yours, &c

B. Cooke.

The Apples shewn
Nov. 14, 1745.

Vol. 45, p. 602.

IV. *A letter from* Benj. Cooke *F.R.S. to* Peter Collinson F.R.S. *concerning a* mixed Breed *of* Apples, *from the Mixture of the* Farina.

Newport, Dec. 4, 1748.

Dear Cousin,

I sent you last Year a Specimen of the Effect of the *Farina* of a rough-coat Apple striking on the Flower of a smooth-coat; I have now sent an Example of the *Farina* of the latter changing the former into its own Dress and Likeness.

The Situation of the Russeting was such, that he was surrounded by Winter Pippins, Pearmains, and such-like; and we put the Master-Fruit together with several of the

Changelings, as they grew on the same Branches mixed together.

This Instance will shew what Alterations may be expected in cognate Species; and I should have given an Example of a kind of Antipathy betwixt the Pear and the Apple in like Circumstances, but was disappointed. I am,

Dear Cousin,
Yours, most obliged and affectionate,
B. Cooke.

(Read Dec. 22, 1748.)

Vol. 46, p. 205-207,—

Part of a letter from Mr. Benj. Cooke, *F.R.S. to Mr.* Peter Collinson, *F.R.S. concerning the Effects of the Mixture of the* Farina *of* Apple-Trees; *and of the Mayze or* Indian Corn: etc.

When the Farina of one Apple impregnates another's Blossom of differing Species, we see the Change in the Fruit; but whether any lasting Impression is left on the Bough which bore it (as seems to be in Tulips and some other Flowers), is not so easy to determine. Experiments of this sort being not to be made at all, but catch'd at distant Opportunities; and till this Point is settled, the Distemper of my good Friend's Tree must rest unexplained.

Artificial Helps of Sight have added to former Discoveries the Explosive Manner of the *Farina's* Action; but what may be the Effect of the inconceiveably fine subtile Matter emitted from its Globules, and continually wafted about in great Plenty and Variety in the Summer Air, not only on vegetable Productions (where on different Subjects it may not improbably have opposite Effects) but other Matters not yet suspected to be so much under its Influence, remains a Field of Inquiry for future Ages. However, to what Mr. *Loggan* hath very justly observed (*Transact.* 440) on the Manner of Impregnation of the Seeds in Mayze—I can add this, that if the Seed and whole Species of Mayze be planted about two Yards Distance from each other, there will be a Mixture of red and white Grains in the Ears of each Plant, and you may with Pleasure observe the Filament in the white Plant, which hath been struck with the red Farina, discovering its alien Commerce by a conscious Blush, and by counting the Threads they stained, foretell how many corresponding Seeds will appear red, at the opening of the Ear, when ripe.

(Read Nov. 2, 1749)

Another reference to Cooke's hybridization is found in a letter of Peter Collinson to Sir Hans Sloane. The following extract is taken from p. 84 of *The Life of Peter Collinson* by *Norman G. Brett-James.*

I enclose an ear of corn from my cousin, Mr. Benjamin Cook, Newport in the Isle of Wight. Pray be careful of the tenderfoot stalks of the wild oats, for the least accident may break them off the ear and then the curiosity is lost. Grain-bearing vegetables are furnished with seed vessels alike, and at the time of flowering, when the impregnation is made, the male farina of the wild oat, which grows in common with the wheat, did impregnate the pistillum of some of the forming grain in the wheat ear, which was very probably the way by which two different grains on the same ear received this formation.

Chronological List of Plant Hybridization before Koelreuter

The following dates may represent either the year in which the hybridization occurred or the earliest record we have of the occurrence. This list is probably very incomplete and new names will doubtless be inserted when the period has been properly investigated.

1716—Cotton Mather reported the crossing of different color varieties in *Zea Mays* and the pollination of *Cucurbita Pepo*, var. *ovifera* by either *C. Pepo* var. *condensa or by C. maxima.*

1717—Thomas Fairchild crossed *Dianthus caryophyllus* with *D. barbatus*, the first recorded artificial hybrid.

1717—Richard Bradley noted spontaneous hybridization in Auricula (*Primula* § *Auricula*). He also discussed the effect of foreign pollen upon the fruit of Pyrus Malus and of *Cucumis.*

1720—William Knowlton found and described a sterile hybrid in the genus *Dianthus.*

1721—Philip Miller described hybridization in *Brassica.*

1724—Paul Dudley described variety crosses in *Zea Mays.*

1729—Thomas Henchman observed variety crosses in *Pisum*, and noted the occur-

rence of different colored peas in the same pod.

1739—John Bartram recorded hybridization in *Lychnis* (*Phlox?*).

1745—Benj. Cooke reported variety crosses (?) in *Pyrus Malus* and in 1749 variety crosses in *Zea Mays*.

1746—J. G. Wahlbom described the crossing of red and white varieties in *Tulipa*.

1750—Johannes Haartman on taxonomic grounds assumed that various forms of *Veronica, Delphinium, Saponaria, Acteae*, etc., were hybrids.

1760—Carl Linnaeus described hybridization in *Mirabilis, Veronica, Delphinium, Heiracium* and *Trapogon*.

1761—Joseph Gottlieb Koelreuter systematically investigated hybridization in *Nicotiana, Kedmia, Dianthus, Matthiola, Hyoscyamus*, etc.

Summary

1. A number of documents describing instances of plant hybridization before Koelreuter (some hitherto unpublished) are here reproduced and made available to modern geneticists.

2. Different colored grains on the same ear of corn were observed and described long before their significance was recognized. This occurrence has been recorded by Tabernaemontanus (1588), Matthiola (1590), Gerard (1597), a New England Journal (1620), Bauhin (1620), Laet (1633), Winthrop (1678), Beverly (1705), Salmon (1710) and Morison (1715).

3. Cotton Mather described hybridization in *Zea* and *Curcurbita* in a letter to James Petiver dated September 24, 1716. The letter is printed in full.

4. The actual finding of the *Dianthus* hybrid by Thomas Fairchild was described by Patrick Blair in 1720.

5. Richard Bradley described hybridization in *Auricula* (*Primula*) in 1717 and discussed the effect of foreign pollen upon several varieties of apples and melons.

6. William Knowlton observed hybridization in *Dianthus*. Our earliest record of this hybrid is 1720.

7. The Rev. Mr. Thomas Henchman observed hybridization in Pisum in 1729. He was especially interested in the occurrence of different colored peas in the same pod.

8. Mr. Benj. Cooke described variety crosses in *Zea Mays* in 1749. Four years earlier he discussed cross pollination in different varieties of apples.

9. Twelve different investigators have been found who described plant hybridization before Koelreuter reported his classical experiments. With the possible exceptions of Fairchild and Linnaeus, they were all extremely casual in their researches and seemed to have been content with describing what they found without resorting to designed experiments. In spite of this, plant hybridization was well understood in 1761.

The author is greatly indebted to Dr. George Strodach for the translation of Tabernaemontanus, Bauhin, Laet and Morison, and to Mr. J. C. Graddon, Assistant in the Library of the Royal Society, for locating the unpublished records of Fairchild, Knowlton and Henchman.

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The Testcross—A Suggested Genetic Term

SINCE about 1912 the term "backcross" has been extensively used, mainly by the Drosophila workers, with the special meaning of the cross of a heterozygote to the corresponding homozygous recessive. This use of the term was largely in linkage studies where an individual heterozygous for two or more loci in the same chromosome was "backcrossed" to the corresponding multiple-recessive homozygote. In more recent years the use has been extended, for example, to cover such a case as that of a female heterozygous for Lobe (a second-chromosome dominant), for lethal-IIax (a recessive lethal in II) and for speck (a second-chromosome recessive) to a male of the constitution IIax sp/Curly:

$$\frac{L \quad + \quad sp}{+ \quad IIax \quad +} \; ♀ \times IIax \; sp/Cy \; ♂$$

The non-Cy offspring would be referred to as constituting a three-point alternated female-backcross series of zygotes. But such usage is a far departure from the prior, less specialized, and still current meaning of the term "backcross" as the cross between a hybrid or heterozygous individual and either one of its *parental* forms (see also current glossary on back inside page of THE JOURNAL OF HEREDITY).

The type of cross designated by the Drosophilia workers as the "backcross" is here to stay, since it offers distinct advantages over F_2 results. The main advantage is that of a parallelism between the zygotic series directly observable and the gametic series which is the real object of study—a parallelism disturbed only when unequal viability of the different zygotic types is encountered. It is our most reliable test of a gametic series, whether many or only one factor difference is involved. Because of this characteristic we may employ the term " testcross" wherever "backcross" has been used in the special meaning, releasing the term "backcross" for use in its original and general meaning.

CALVIN B. BRIDGES.

THE PROGRESS OF EUGENIC STERILIZATION

PAUL POPENOE
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MORE than 150,000,000 people are now living under laws providing for eugenic sterilization in selected cases. The operations used for sterilizing patients, without unsexing them, are of relatively recent origin. It is true that as long ago as 1823, Dr. James Blundell read a paper before the Medico-Chirurgical Society of London in which he suggested that the fallopian tubes, which conduct the egg cell from the ovary to the uterus, might be cut at the time of a Cæsarean operation, thus preventing future pregnancy. But so far as is known, Dr. Blundell never performed such an operation, but merely offered it as a possibility.

In 1881 Dr. S. S. Lungren of Toledo, Ohio, tied the tubes of a patient with silk at the time of a Cæsarean section in order to prevent future pregnancy. Such a ligature is still used sometimes, but since about one case in five ends in failure, it cannot be regarded as good surgical practice.

In 1891 Dr. A. Crimail, a French surgeon, not only tied the tubes of one of his patients, but also cut them, thereby introducing the modern practice of salpingectomy. With many minor variations of technique, this is the standard operation for the sterilization of the female up to the present day.

Dr. Crimail's patient was sterilized during a Cæsarean section in order to prevent the risk of further childbirth. The first recorded sterilization operation for eugenic reasons was that performed in 1897 by Dr. F. A. Kehrer, a German surgeon, who urged the use of this operation not merely to prevent future Cæsarean sections, but to prevent future childbirth when such a child-birth would be undesirable. He published the details of an operation he had performed on a woman who had given birth to seven children, some of whom were feeble-minded, others abnormal in other ways. This report, with its definite championship of eugenic sterilization, attracted widespread attention. Surgically the modern sterilization of the female, for eugenic reasons, may be said to date from the year 1897.

The sterilization of the male dates from the same year. Experiments in tying off the vas are reported as early as 1785, but the first operation of vasectomy is credited to Dr. H. G. Lennander of Upsala, Sweden, who in 1897 published a discussion of vasectomy and described his use of it in dealing with enlargement of the prostate gland.

Apparently it was not until two years later that the operation was first used for eugenical purposes. This was due to Dr. Harry Sharp of the Indiana State Reformatory at Jeffersonville. Since Dr. Sharp began to operate on men who had been legally committed to the institution, the year 1899 is universally accepted as the beginning of official eugenic sterilization and Dr. Sharp is unquestionably entitled to credit as the father of this procedure.

At that time, Indiana had no law to deal with such operations and Dr. Sharp proceeded merely by getting the consent of the inmates. He performed several hundred operations within a few years and the reports on these began to give a substantial basis for legislation. The legislature of Pennsylvania was the first one to pass a

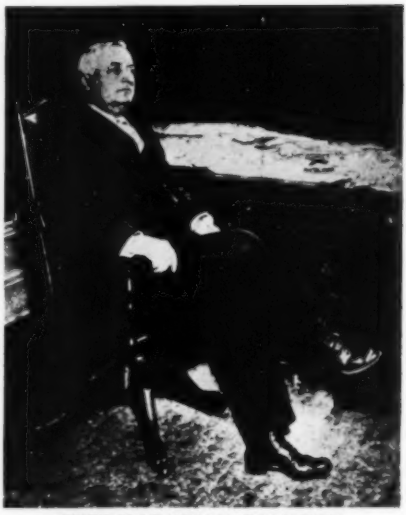
sterilization law in 1905, but this was vetoed by the Governor, leaving to Indiana the distinction of being the first state to put a sterilization law on its statute books, in 1907.

In the next legislative year, that of 1909, Washington and California both followed this example by the adoption of sterilization laws only a few weeks apart. The Washington law has remained virtually unused ever since.

The California law was introduced on February 8, 1909, as a bill by Senator W. F. Price, of Santa Rosa, Calif. It passed the Senate on March 16 with 21 ayes and 1 no, and passed the House on March 22 with 41 ayes and not a single vote recorded against it. It was approved on April 26 by Gov. James N. Gillett and became a law on June 25, 1909.

The drafting and adoption of this bill, virtually without opposition, form a permanent monument to Dr. F. W. Hatch who was Secretary of the State Lunacy Commission and later given the title of General Superintendent of State Hospitals. For nearly a quarter of a century Dr. Hatch was virtually in charge of the state's measures and machinery for dealing with the insane and feeble-minded. The adoption of sterilization as a state policy was one of his greatest interests and he followed the administration of this law with the closest attention until his death on February 24, 1924, at the age of 74.

The conservative, sympathetic, and intelligent administration of the California law which was carried out by Dr. Hatch and the various medical superintendents of the state hospitals and institutions, gave the application of sterilization in this state a long lead, so that even up to the present California has performed more sterilizations than all the other states combined. Other states have continued to fall in line with these precedents, the 27 states that now have eugenic sterilization on their statute books being as follows with the year in which the first law was adopted in each case:



STERILIZATION PIONEER

Figure 4

Dr. F. W. Hatch is credited with drafting the California sterilization statute; and after its almost unanimous adoption by the legislature he had charge of its enforcement during the early years. Largely on account of the Hatch's administration of the law during its experimental stages, more sterilizations have been performed in California than in all the other states combined.

Alabama	1919
Arizona	1929
California	1909
Connecticut	1909
Delaware	1923
Idaho	1925
Indiana	1907
Iowa	1911
Kansas	1913
Maine	1925
Michigan	1913
Minnesota	1925
Mississippi	1928
Montana	1923
Nebraska	1915
New Hampshire	1917
North Carolina	1919
North Dakota	1913
Oklahoma	1931
Oregon	1917
South Dakota	1917
Utah	1925
Vermont	1931
Virginia	1924
Washington	1909
West Virginia	1929
Wisconsin	1913

STUDENT OF STERILIZATION

Figure 5

Long interested in philanthropy, and in race-improvement, Mr. E. S. Gosney initiated in 1925 a study of the actual effects of the California law during the fifteen years it had been in operation. This study has continued since that time, and has made available more facts about the effects of such a law than are elsewhere to be found. To provide for the continuance of this work Mr. Gosney has incorporated and endowed the Human Betterment Foundation.

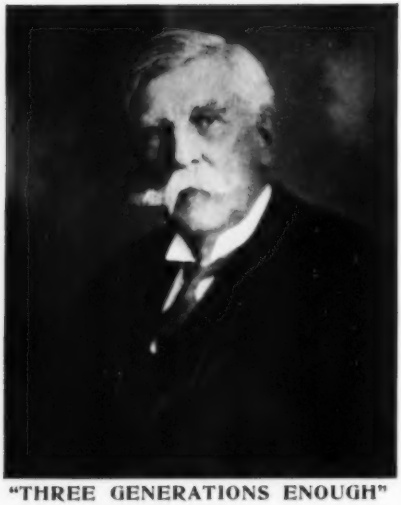
The province of Alberta, Canada, adopted a sterilization law early in 1928 and British Columbia took a similar step in 1933. Denmark has had a sterilization law since June, 1929, the Canton of Vaud in Switzerland since January 1, 1929 and the state of Vera Cruz in Mexico since December 1, 1932. Finally the adoption by the German government on July 26, 1933 of a comprehensive sterilization law, taking effect on January 1, 1934, brought under this measure the largest number of persons who had ever been included in the scope of such legislation at any one time.

From the very beginning, the progress of eugenic sterilization has been attentively followed by competent students. Since the founding of the Eugenics Record Office in 1910 its Superintendent, Dr. Harry H. Laughlin, has devoted particular attention to this subject and his many publications have presented a record of the progress of legislation and litigation, together with a wealth of detail on other aspects of the problem.

With the widespread discussion given to sterilization, however, there was still a lack of adequate information as to the actual effects of the operation and the circumstances surrounding the enforcement of a sterilization law. The only large and satisfactory body of evidence was that which was being steadily increased by the institutions of California, and the study of such a large body of material was an undertaking far beyond the reach of any ordinary individual or institution.

Meanwhile, E. S. Gosney of Pasadena who, during a long career as lawyer, banker, live stock breeder, educator, and philanthropist, had given particular thought to the application of eugenic principles in society, began a search for some definite project of eugenic value to which he could devote some of his time, energy, and accumulated wealth. After years of consideration of various possibilities, he decided nothing would be more worth while than a thorough-going and impartial study of the workings of California's eugenic sterilization law. If the abundant data available in the state institutions were collected, tabulated, analyzed by the most refined statistical methods and made public, he believed that the whole question would be put on a different plane. Discussion of sterilization would then no longer have to be based, as previously, on what people hoped it might do, or feared it might do, but could be based on the actual results in one state of thousands of cases extending over a score of years.

Mr. Gosney therefore organized the study of the California results in 1925, with the aid of a group of consultants representing various specialties, and the



"THREE GENERATIONS ENOUGH"

Figure 6

Mr. Justice Oliver Wendell Holmes of the U. S. Supreme Court wrote in 1927, the opinion of that Court upholding the constitutionality of a properly safeguarded sterilization law as a social measure. Its fair-minded balancing of the somewhat conflicting claims of the individual and of society render this decision an important document in the literature of eugenics. For the text of the opinion see page 25.

actual investigation was begun on March 1, 1926, with the hearty cooperation of all the state authorities. This has continued up to the present time and will, no doubt, be carried on for some years to come. A complete survey of the first 6,000 cases was published in a score of technical papers in various scientific journals, and then embodied in a more popular book by E. S. Gosney and Paul Popenoe entitled, "Sterilization for Human Betterment,"* of which translations have been issued in Berlin, Tokyo, and Paris. Meanwhile, in order to provide for the continuance of this study, Mr. Gosney had created the Human Betterment Foundaiton and endowed it liberally. The date of incorporation was February 2, 1929.

Some of the American laws were hastily drawn without adequate knowledge of what was needed, and in several cases were so defective or unreasonable that they were held unconstitutional by the state courts.

Test of the Virginia Law

In 1924 Virginia adopted a sterilization measure and on September 10th of that year Dr. A. S. Priddy filed a petition with his Board of Directors, in accordance with the law, for the sterilization of one Carrie Buck, an inmate of the State Colony for Epileptics and Feeble-minded of which he was Superintendent.

Carrie Buck was then 18 years old with a mental age of 9. Her mother, then 52 years old with a mental age of 7 years 11 months, had a long record of immorality, prostitution, and dependency. She had three children, as to the paternal parentage of all of whom there was considerable doubt. One of these, Carrie Buck, had been adopted by a family in Charlottesville. She attended school five years and attained the sixth grade, helped with the domestic work of the household, but proved to be incorrigible and finally gave birth to an illegitimate child. The family felt it could no longer manage her, and had her committed to the State Colony as feeble-minded. The commencement of proceedings to sterilize her initiated the most important case in the history of eugenic sterilization.

While this litigation was in process Dr. Priddy died and his successor, Dr. James H. Bell, was substituted in the case. It was desirable to have the Virginia law passed on by the courts before it was put into effect and the litigation in which Carrie Buck was the plaintiff, and which is now historical under the title of *Buck vs. Bell*, was arranged as a test case by the officials of the State Colony. The Board of Directors having issued an order for the sterilization of Carrie Buck, her guardian appealed the case to the Circuit Court of Amherst County, which

*Macmillan, New York, 1929.

BUCK VS. BELL

Figure 7

Carrie Buck was committed to the Virginia State Colony for feebleminded, near Lynchburg. As a test case to determine the constitutionality of the law, she was selected for sterilization and an action brought in her name against the superintendent of the colony, to prevent the operation. This case, carried to the Supreme Court of the United States under the title of *Buck vs. Bell* led to the decision written by Justice Oliver Wendell Holmes, fully upholding the constitutionality of eugenic sterilization. At the left, above, Dr. J. H. Bell, superintendent of the state Colony; at right, Carrie Buck.

sustained the decision of the Board. It was then carried to the Supreme Court of Appeals of Virgina, which sustained the law as a valid enactment under the state and federal constitutions. It was then carried up to the Supreme Court of the United States, which on May 2, 1927, handed down the decision written by Justice Oliver Wendell Holmes upholding the constitutionality of the statute. In the course of this decision Justice Holmes recorded the memorable opinion that "Three generations of imbeciles are enough." (See page 25.)

Carrie Buck's sterilization was thereupon carried out and she was later paroled from the Colony. Her illegitimate child, which had been previously placed out for adoption, later died.

The decision in *Buck vs. Bell* settled for all time the constitutionality of eugenic sterilization in the United States. Together with the evidence published by the Human Betterment Foundation, it tended to encourage sterilization in

FIRST EUGENIC STERILIZER

Figure 8

Dr. Harry Sharp, of the Indiana State Reformatory, performed the first eugenic sterilizations in 1899. At that time no law existed authorizing such procedure, and Dr. Sharp performed several hundred operations by merely getting the consent of the inmates. His experience formed the basis for the first state sterilization law, passed by Pennsylvania in 1905.

HISTORIAN OF STERILIZATION

Figure 9

Dr. Joseph Mayer has recently reviewed in detail the history of the sterilization movement. His tentative conclusion that selective sterilization was in accord with the traditions of the Catholic Church was published before the Papal encyclical of December 31, 1930. This document placed that Church officially on record as being opposed to such operations.

many states which had theretofore been waiting for further guidance. The latest complete tabulation of official sterilizations in the United States is that published by the Human Betterment Foundation as of January 1, 1933, as follows:

State	Male	Female	Total
Alabama	73	58	131
Arizona	10	10	20
California	4,423	4,081	8,504
Connecticut	18	320	338
Delaware	181	115	296
Idaho	4	9	13
Indiana	159	58	217
Iowa	56	38	94
Kansas	588	388	976
Maine	5	36	41
Michigan	264	819	1,083
Minnesota	72	621	693
Mississippi	1	11	12
Montana	33	48	81
Nebraska	94	135	229
New Hampshire	23	142	165
New York	1	41	42
North Carolina	10	36	46
North Dakota	56	37	93
Oklahoma	0	0	0
Oregon	296	586	882
South Dakota	55	84	139
Utah	44	41	85
Vermont	8	22	30
Virginia	479	854	1,333
Washington	6	24	30
West Virginia	0	1	1
Wisconsin	40	452	492
Total	6,999	9,067	16,066

Sterilization in Germany

The progress of sterilization abroad had been largely in the field of discussion until the action of Germany brought a large territory under the scope of such a measure. In no coun-

tries has the discussion of sterilization been so active as in Germany and Great Britain, and the literature on the subject, in the first named country in particular, is now voluminous.

The history of the whole movement has been reviewed in detail by Dr. Joseph Mayer, a Roman Catholic theologian and social worker whose bibliography alone (confined almost wholly to works in the German language appearing prior to 1927) covers 31 pages. Dr. Mayer also went fully into the ethical and theological aspects of the question, which at that time had never been the subject of any authoritative pronouncement by the Roman Catholic Church. His tentative conclusion was that selective sterilization for eugenic purposes, properly administered, was entirely in accord with the traditions and principles of the Roman Catholic religion. This conclusion was superseded by the Papal encyclical, *Casti connubii*, of December 31, 1930 which held sterilization for any except therapeutic reasons, not permissible for Roman Catholics.

Sterilization is only one of many important measures for dealing with the problems of mental deficiency and mental diseases. But as a practical preventive measure, it seems likely to take a place in the humanitarian program of every civilized country in the near future. Some careful observers believe that the Soviet Union and Japan are likely to make wider use of it than other countries because of their individual problems.

Meanwhile, the use of sterilization in private surgical practice is spreading widely, steadily, and rapidly. Naturally there are no means of getting information about the exact extent of this private practice, but all indications are that it is much larger than is generally supposed.

Supreme Court's Opinion in Buck Case

The decision of the U. S. Supreme Court in the case of Buck vs. Bell *is perhaps the most important legal document in the history of sterilization. It is therefore being reprinted in full.*—EDITOR.

Mr. Justice Holmes delivered the opinion of the Court:

This is a writ of error to review a judgment of the Circuit Court of Amherst County, by which the defendant in error, the superintendent of the State Colony for Epileptics and Feeble Minded, was ordered to perform the operation of salpingectomy upon Carrie Buck, the plaintiff in error, for the purpose of making her sterile. *134 Va. 310.* The case comes here upon the contention that the statute authorizing the judgment is void under the Fourteenth Amendment as denying to the plaintiff in error due process of law and the equal protection of the laws.

Carrie Buck is a feeble minded white woman who was committed to the State Colony above mentioned in due form. She is the daughter of a feeble minded mother in the same institution, and the mother of an illegitimate feeble minded child. She was eighteen years old at the time of the trial of her case in the Circuit Court, in the latter part of 1924. An Act of Virginia, approved March 20, 1924, recites that the health of the patient and the welfare of society may be promoted in certain cases by sterilization of mental defectives, under careful safeguard, &c.; that the sterilization may be effected in males by vasectomy and in females by salpingectomy, without serious pain or substantial danger to life; that the Commonwealth is supporting in various institutions many defective persons who if now discharged would become a menace but if incapable of procreating might be discharged with safety and become self-supporting with benefit to themselves and to society; and that experience has shown that heredity plays an important part in the transmission of insanity, imbecility, &c. The statute then enacts that whenever the superintendent of certain institutions including the above named State colony shall be of opinion that it is for the best interests of the patients and of society that an inmate under his care should be sexually sterilized, he may have the operation performed upon any patient afflicted with hereditary

forms of insanity, imbecility, &c., on complying with the very careful provisions by which the act protects the patients from possible abuse.

The superintendent first presents a petition to the special board of directors of his hospital or colony stating the facts and the grounds for his opinion, verified by affidavit. Notice of the petition and of the time and place of the hearing in the institution is to be served upon the inmate, and also upon his guardian, and if there is no guardian the superintendent is to apply to the Circuit Court of the County to appoint one. If the inmate is a minor, notice also is to be given to his parents if any with a copy of the petition. The board is to see to it that the inmate may attend the hearings if desired by him or his guardian. The evidence is all to be reduced to writing, and after the board has made its order for or against the operation, the superintendent, or the inmate, or his guardian, may appeal to the Circuit Court of the County. The Circuit Court may consider the record of the board and the evidence before it and such other admissible evidence as may be offered, and may affirm, revise, or reverse the order of the board and enter such order as it deems just. Finally any party may apply to the Supreme Court of Appeals, which, if it grants the appeal, is to hear the case upon the record of the trial in the Circuit Court and may enter such order as it thinks the Circuit Court should have entered. There can be no doubt that so far as procedure is concerned the rights of the patient are most carefully considered, and as every step in this case was taken in scrupulous compliance with the statute and after months of observation, there is no doubt that in that respect the plaintiff in error has had due process of law.

The attack is not upon the procedure but upon the substantive law. It seems to be contended that in no circumstances could such an order be justified upon the existing grounds. The judgment finds the facts that have been recited that Carrie Buck "is the probable potential parent of socially inadequate offspring, likewise afflicted, that she may be sexually sterilized without detriment to her general health and that her welfare and that of society will be promoted by her sterilization," and thereupon makes the order. In view of the general declaration of the legislature and the specific findings of the Court obviously we cannot say as a matter of law that the grounds do not exist, and if they exist they justify the result. We have seen more than once that the public welfare may call upon the best citizens for their lives. It would be strange if we could not call upon those who already sap the strength of the state for these lesser sacrifices, often not felt to be such by those concerned, in order to prevent our being swamped with incompetence. It is better for all the world, if instead of waiting to execute degenerate offspring for crime, or to let them starve for their imbecility, society can prevent those who are manifestly unfit from continuing their kind. The principle that sustains compulsory vaccination is broad enough to cover cutting the Fallopian tubes. *Jacobson v. Massachusetts*, 197 U. S. 11. Three generations of imbeciles are enough.

Syllabus

But it is said, however, it might be if this reasoning were applied generally, it fails when confined to the small number who are in the institutions named and is not applied to the multitudes who are outside. It is the usual last resort of constitutional arguments to point out shortcomings of this sort. But the answer is that the law does all that is needed when it does all that it can, indicates a policy, applies it to all within the lines, and seeks to bring within the lines all similarly situated so far and so fast as its means allow. Of course so far as the operations enable those who otherwise must be kept confined to be returned to the world, and thus open the asylum to others, the equality aimed at will be more nearly reached.

Judgment affirmed.

Mr. Justice Butler dissents.

The Inheritance of Tuberculosis

ZWILLINGSTUBERKULOSE, *by* KARL DIEHL and O. F. v. VERSCHUER. VIII + 500 pp., 100 figs., 50 tables. Jena, Verlag von Gustav Fischer. 1933.

NO method of research in human heredity has been more productive of interesting results than the study of twins. This volume, the joint product of a physician and a geneticist, records the application of the method to the problem of the inheritance of tuberculosis. The authors believe that tuberculosis can not be genetically analyzed with sufficient clarity by the family history method, because of its dependence on the coincidence of man

and bacteria, both of which are subject to genetic and environmental variations which affect the development of the disease. They therefore turn to an analysis of 127 pairs of twins, 45 of which are pairs of identical twins, 53 are pairs of like-sexed fraternal twins, and 29 are pairs of fraternal twins of unlike sex.

The volume opens with a discussion of the literature and the various theories in regard to the development of tuberculosis. Then follows a discussion of the biology of twins and the value of twins in the study of heredity.

The twins used in the investigation were studied from many standpoints. It is recorded that the members of the pairs of identical twins were the same in every case as to blood group, the M-N reaction, eye color, hair color (with some minor differences), hair form and skin color, and agreed in form of nose, lips, ears and eyebrows. These characters were frequently different in the members of the pairs of fraternal twins.

The bulk of the book, extending through pages 165-422, is given over to case histories, with x-ray photographs, and pictures of some of the twins. Following this, the conclusions are given in some detail. The authors conclude that susceptibility to tuberculosis is inherited in a relatively simple manner, and that from the same genetic make-up various tubercular syndromes may develop. These conclusions are based on the following results of the investigation:

Of seven pairs of twins in which in spite of a common exposure only one member of the pair contracted the disease, six pairs were fraternal twins. Of nine pairs in which one member had the disease and the other member remained free from infection, eight were fraternal twins. Of twelve pairs of twins from healthful surroundings and with identical intra-pair environments, five pairs showed tuberculosis in one twin. These five were all fraternal twins. Seven pairs showed tuberculosis in both twins. Five of these seven pairs were identical twins.

Where the environment was dissimilar, tuberculosis was found in both members of the pair only in cases of identical twins, never in fraternal twins.

In 106 pairs of twins showing tuberculosis in one or both members, 70 per cent of the identical pairs showed tuberculosis in both members, while only 25 per cent of the fraternal pairs showed tuberculosis in both members. Among members of identical twin pairs, clear concordance as to tubercular changes is twice as common as disconcordance, whereas among members of fraternal twin pairs disconcordance is fourteen times as common as concordance. With increasing difference in the form of tuberculosis within the pair, there is a decreasing proportion of identical twin pairs.

The authors also conclude that the difference between identical twins and fraternal twins in reference to tuberculosis is entirely a matter of the identical inheritance of the identical twins on the one hand, and the difference in inheritance of the fraternal twins on the other; that no anatomical characteristics can be found to be correlated with disposition to tuberculosis; that a specific and hereditary predisposition is the primary basis for reacting pathologically to the bacillus; and that about one fifth of those who inherit the disposition remain well in spite of it, mainly because of inheritable modifying factors.

The final chapter of the book is given over to a rather involved discussion of eugenics and the application of eugenic measures to the tuberculosis problem.

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BRANCHED SILKLESS EARS

Figure 10

Branched silkless ears are of two types: those with branches extending to the tips (left), and those with an unbranched central spike (center). At the right is shown an ear from a plant carrying the genes both for branched silkless and for ramose. The gene for ramose produces branched ears but does not suppress the silks.

HERITABLE CHARACTERS IN MAIZE

XLVII—Branched Silkless

J. H. KEMPTON
U. S. Department of Agriculture

BRANCHED silkless designates a heritable recessive character in maize in which the ears are greatly ramified as in ramose but are wholly without silks as in the silkless form described by Jones.[2] From the appearance of the ears alone one would conclude that they represent the double recessive combination of ramose and silkless. Breeding tests, however, furnish conclusive evidence that only a single gene is involved in the expression of branched silkless and that this gene is distinct from the ramose and from the silkless genes. It is proposed to assign the symbols *Bd bd* to the gene for branched silkless and its allelomorph.

The tassels of branched silkless, though abnormal, are not ramified as is the case with plants of the ramose character. The division of the tassel into branching portion and central spike is as definite as in normal plants. Branched silkless, like normal plants, have a central spike somewhat longer than the branching portion and the uppermost branch.

The tassels usually have a thickened appearance suggesting those of pod corn and many plants shed pollen sparingly. In most F_2 progenies we have experienced little difficulty in identifying the branched silkless plants by means of tassel characteristics alone. The thickened appearance of the tassel results from the development of short branches in place of paired spikelets but these branches fail to show the decrease in length, characteristic of ramose tassels, as the tip of the tassel is approached. On central spikes these branches may be two or three centimeters long and bear six to ten spikelets. On the main branches there appears to be more of a tendency to retain the pediciled and sessile form. However, in place of the pediciled spikelet there may be a short branch 5 mm. or so long bearing two to four spikelets and in such case the sessile spikelet is represented by a similar group of two to four spikelets.

This branched silkless character was first called to our attention by J. M. Hammerly of the Office of Corn Investigations, Division of Cereals, Bureau of Plant Industry. He stated it was discovered by E. B. Brown, formerly of the same office. Subsequently what appears to be the same character has been found, after one generation of selfing, in a strain of sweet corn received through A. E. Longley from Nova Scotia.

Mr. Hammerly kindly permitted us to use pollen from his plants and most of our investigations involving this character have been conducted with his stock. We have not succeeded in testing the genetic identity of the two forms of branched silkless because of the great difference in season, between the two strains.

Both forms behave as simple recessives in the crosses made thus far with normal plants. In crosses with non-branched silkless* the first generation is normal, all plants developing silks in the ordinary manner. The silkless used in these crosses apparently differs from that described by Jones in that it shows no linkage with liguleless but no direct test for identity has been made. It was derived

*These crosses, of course, can be made only with one parent heterozygous for the silkless character with the result that only one-half of the F_1 plants are actually crosses between the two forms of silkless.

TASSEL AND EAR TYPES PRODUCED BY INTERACTION OF BRANCHED SILKLESS AND OTHER GENES

Figure 11

A—The "cauliflower" ears and tassel produced by the interaction of the genes for branched silkless and tunicate; *B*—Normal branched silkless. Note the short thick branches of the tassel characteristic of these plants. *C*—Branched silkless and ramose. *D*—Branched silkless and adherant, showing development of silks in the tassel.

from the commercial variety, Laguna, grown in the Southwest and is widespread, having been isolated from stocks of this variety from Mississippi, New Mexico and Arizona. In the second generation of the crosses between branched silkless and non-branched silkless only three sorts of plants are obtained—normal, silkless and branched silkless in the ratio of 9:3:4. The double recessive evidently is indistinguishable from branched silkless plants.

In common with ramose the first generation is also normal with no indication of the ramose or branched silkless type of branching in either ears or tassels. In the second generation of crosses with ramose the double recessive combination, ramose-branched silkless, is identifiable from the tassels which have the characteristic appearance of ramose tassels plus the thickened aspect of branched silkless tassels.

Curiously enough in crosses with pod corn the combination branched silkless and pod that appears in the second generation often closely resembles the "Cauliflower" inflorescence obtained in the F_2 crosses between pod and ramose.[1]

Although the excessive production of branches by branched silkless plants seems to be constitutional, as is evidenced by the development of branches on both ears and tassels, the silkless condition seems to be more restricted. At least branched silkless plants having silks in their tassels have been found in the second generation of certain crosses leading to the suggestion

A BRANCHED SILKLESS EAR

Figure 12

Ears of this type have been found not unpalatable as a salad though perhaps somewhat too sweet for many tastes.

Branched silkless and adherant, showing development of silks in the tassel.

that in crosses between "tassel seed" and this character the double recessives will produce silks in the tassels but not on the ears. Such crosses have been made but the segregating generations are yet to be grown.

Jones[3] has shown that the action of the silkless gene (*sk*) is inhibited by that of the tassel seed gene. He found in crosses between these two forms, the double recessive combination of tassel seed and silkless, that appears in the second generation, develops silks not only in the tassels, but in the ears as well.

Branched silkless has been crossed with anther ear and brachytic in chromosome 1; with liguleless in chromosome 2, with tunicate in chromosome 4, with yellow endosperm in chromosome 6; with golden and lineate in chromosome 10 and with a non-branched silkless and with fasciated ears, both these latter unlocated.

The distributions in the segrating populations are shown in Table I.

In none of the populations, with the exception of the F_2 of the tunicate cross, is there any evidence of linkage. The single second generation tunicate cross with a X^2 of 7.4 has an indicated crossover percentage of 28.5 $\pm$ 4.7 but a subsequent backcrossed progeny fails to substantiate this linkage. Both populations are small and it remains a possibility that *Bd Bd* is on the chromosome with *Tu Tu*.

Branched silkless is strictly a mature plant character but its definite differentiation from normal plants and its easy recognition from tassel characteristics may make it useful in genetic analysis despite the disadvantage of female sterility.

TABLE I.—Segregating populations of crosses between branched silkless and the characters designated in the left hand column.

	Bd X	Bd x	bd X	bd x	X^2
Brachytic	323	110	103	38	.197
Golden	125	31	46	9	.163
Lineate	399	121	133	45	.375
Yellow endosperm	318	107	93	33	.059
Ramose	210	66	78	14	3.07
Anther Ear	140	34	30	4	1.14
Liguleless*	87	38	104	32	1.57
Silkless	221	65	101	0	1.03†
Fasciated ear (backcross)	69	67	67	49	4.3
Tunicate	61	37	16	11	7.4
Tunicate (backcross)	24	28	18	25	.17

* Segregating 1:1 for branched silkless.
† Calculated for 9:3:4 ratio.

Literature Cited

1. Collins, G. N., Hybrids of Zea ramose and Zea tunicata. *Jour. Agr. Res*, Vol. IX, No. ii, 1917.

2. Jones, D. F., Heritable Characters of Maize. XXIII Silkless. *Jour. of Heredity*, XVI, 339-341.

3. ———— Dioecious Maize. *Science*, Vol. 73, p. 432. 1931.

▼

Peculiarities of Hair Growth

DIE BEHAARUNG DES MENSCHEN, *by* Scheuer, O., (The hair cover of man). Monographien zur Frauenkunde und Konstitutions forschung Nr. 17. 93 pages, 17 text figures, 6 marks. Leipzig, 1933.

THIS is a short, non-technical survey of the normal peculiarities of hair growth in man, its racial and secondary sexual differences, evolutionary history, physiological functions, and its rôle in the relationship of the two sexes, as well as of its abnormalities due to endocrine disturbances, tumors and other pathological agencies.

W. Landauer.

THE DIAGNOSIS OF MONOZYGOTIC TWINNING

H. S. REICHLE
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THIS discussion will be confined to a consideration of Newman's opinions on the diagnosis of monozygotic twinning; in particular I shall consider his criticisms of my papers (*Biological Bulletin* Vol. 56, No. 3 and No. 5). Since the subject has become very involved and all the more so because Newman has obviously misunderstood my argument, it will not be amiss to review the papers of both Newman and myself.

My interest in twinning was aroused in the year 1928 by observations on apparently identical twins who were patients on the wards of Babies and Childrens Hospital. A systematic study of these infants according to the scheme of Siemens aroused perplexing doubts and led me to examine the limits of error inherent in the method of diagnosis. In order to start investigations on tolerably certain ground I was forced to seek accepted criteria of mono- and dizygotism. There is no doubt whatsoever that combined twins (cosmobia) are monozygotic. Therefore differences shown by the dermatoglyphic method in these subjects should, I thought, demonstrate that palmar and plantar ridges need not be "identical" or even similar in individuals of the same genetic constitution. In no wise did I rest my argument on these grounds alone, nor did I set up the cosmobia as "a norm for diagnosing monozygotic twins in general." At no time did I even imply that "conjoined twins might be expected to show closer resemblances than do separate twins." (NEWMAN—JOURNAL OF HEREDITY, Vol. 22, No. 7, p. 204.) On the contrary I simply doubted that the differences which might exist between cosmobia were greater than those which obtain between "a certain percentage of mono-ovular twins," an opinion which Newman himself accepts in his conclusion to the paper mentioned above (p. 215). There is no escape from this assumption since no sharp division exists between conjoined and non-conjoined twins but rather a series ranging from those who are united completely along the midline, through those united by a mere isthmus of tissue to those born completely separate. (REICHLE—*ibid*, No. 5, pages 316-317.)

My investigations were extended to the palm prints of *unselected* normal twins; here, too, the lack of uniformity proved to my satisfaction that a strictly objective interpretation did not permit an unequivocal diagnosis of monozygotism. I used two criteria with which I controlled the diagnosis obtained with dermatoglyphics and the Siemen's identity test. The one—sex—appears still to be beyond question; no one can assert that twins of different sexes are products of a single ovum. I therefore repeatedly insisted that "more work must be done with *unselected* groups that include both like and unlike sexed twins" (REICHLE—*ibid*, No. 5, p. 324.) Unfortunately this criterion has been neglected by practically all the workers who insist that the diagnosis of monozygotism is accurate. My second criterion was the character of the birth membranes. Verscheur, Siemens, Curtius and Newman have repeatedly attacked its value and asserted that monozygotic twins may have dichorionic membranes.

Newman not only accepts my conclusion that cosmobia differ in their palm and plantar prints but goes beyond this and demonstrates striking differences in the body build and facial expressions of living "Siamese twins." He believes that conjoined twins are the least similar of all monozygotic

twins. To explain this phenomenon he supposes that the separation of the primordial germ mass which leads to twinning may occur at different stages of embryonal development. If the time of separation is delayed until the establishment of axis symmetry, the characteristics of normal bilateral asymmetry are segregated in the twins. Hence cosmobia may differ by as much as the two halves of a normal individual. He interprets the partial or complete asymmetry of dermatoglyphics in cosmobia as indications of such segregation. Thus:

Case T. 61 (Reichle)
Right twin
(R) 10.7.6.5./0.0.0.Ac.L.C13.
(L) 9.X.5"x3./0.0.0.Ac.(C13)
Left twin
(R) 9.X.5.3./0.0.0.Ac.Ac.C13.
(L) 9.X.5.3./0.0.0.Ac.Ac.C13.

He makes the important admission that the same discrepancies may be found in *some* normal monozygotic twins. (See picture on page 48 of the JOURNAL OF HEREDITY Feb., 1931, Vol. 22, No. 2; also page 207 *ibid*, No. 7: "Again if twinning comes very late in gastrulation, it may be possible for division into separate twins to occur, but certain asymmetry fixations may have gone so far that one twin can show only right handed expressions of certain characters on both sides of the body, and the other twin only left handed.") Hence according to his views there must be three types of twins:

1. Dissimilar dizygotic twins.
2. Dissimilar monozygotic twins. (Conjoined twins are included in this group).
3. "Identical" monozygotic twins.

The critical distinguishing characteristic between group 1 and group 2, according to Newman, is the segregation of dermatoglyphic characteristics. (*Ibid*, No. 7, p. 215—"Some of the latter [dizygotic twins] . . . may be as similar in most common features as are a few of the most different monozygotic twins. But the latter will be identifiable as such through a study, especially in connection with dermatoglyphics, of the peculiar workings of the asymmetry mechanisms heretofore described.")

The value of Newman's work rests upon three assumptions:

(1) That the delayed separation of the primordial mass is consistent with accepted theories of embryonal development;

(2) That bilateral symmetry and asymmetrical reversal in dermatoglyphics in the form described by Newman are found exclusively in monozygotic twins:

(3) That embryologists change their views on the importance of the birth membranes as an indication of the type of twinning.

I cannot adequately discuss points (1) and (3) and leave the final decision to the workers in the field of embryology. Newman is apparently willing to abandon an interpretation of foetal membranes which has hitherto been generally accepted by embryologists, solely because the diagnosis made by an examination of the chorion frequently disagrees with that obtained by the technique of Siemens. This I regard as unjustified and in principle erroneous.

There is no doubt that Newman's hypothesis of delayed separation is ingenious and explains many of the differences in twinning. My only objection—aside from those which may be raised by embryologists—is based on the fact that another and far simpler explanation is possible—one which I suggested in my papers but which Newman overlooked and hence failed to consider. Schatz in 1884 described anastomoses which exist between the placentae of monochorionic twins. They represent a third circulation, a shunt by which the blood supply of twins is united in one reservoir. Hence the twin with the greater pressure of circulation will develop at the expense of the other and draw more than his designed allotment of arterial blood, whereas his fellow secures an ever increasing amount of venous blood. The gain in nutrition increases his cardiac power and hence again his pressure of circulation; thus an ever increasing difference in environment is established for the twins at an early and critical

stage of their lives. The one twin may be completely stunted and degenerate into a foetus papyraceous; or an acardiac monster coupled with a twin who has a congenital hypertrophy of the heart may develop. Even in normal monochorionic twins the differences in weight and body length are greater than those found in dichorionic twins (STRASSMANN—Die mehrfache Schwangerschaft in *v. Winkel's Handbuch der Geburtshilfe*, Vol. 1).

The effect must certainly be considered in any study of Siamese twins. That it has been neglected by Newman is all the more remarkable, since on page 204 (JOURNAL OF HEREDITY, Vol. 22, No. 7) he recognizes that the differences between the autosite and the parasite of cosmobia are due to "inhibited development." He continues to say "Such twins may be thought of as ranking at one end of a twinning series, the opposite extreme being the most strikingly similar separate identical twins." Does it therefore not appear logical that the same inhibition of development—which is certainly not a genetic factor—will play some rôle in the rest of the series? It is interesting to note that Verschuer who is a very enthusiastic defender of Siemen's method does not accept Newman's theory of delayed separation, nor does he consider bilateral symmetry of value as a diagnostic aid. (*Zt. f. induktive Abstammungs und Vererbungslehre*, Vol. 6', No. 2.)

Newman also disagrees with my statement that the epidermal ridges, the substratum of palm and plantar formulae, appear late in foetal life. He gives no authority for his belief that asymmetry "in palm patterns is fixed, or at least biased rather early," whereas I had directed attention to the work of Schlagenhaufen who found that the epidermal ridges did not appear before the fourth month of foetal life and to Schultz who found no trace of them in Guinea howling monkeys at the beginning of the second stage of their development (REICHLE—*ibid.*).

In my papers I said that it was not possible to divide a number of *unselected* twins into two sharply differentiated groups corresponding with monozygotic twins on the one hand and dizygotic on the other. My results were confined to children but I also expressed doubt whether the principles of the method had been fully verified even for adults. It does not seem that Newman is very far from this sceptical attitude since he says, "The majority of workers with human twins have had great difficulty in arriving at a clean-cut classification of their cases into monozygotic and dizygotic pairs. It is undoubtedly true that, if the similarity diagnosis is followed out rigidly, unless it be carried out to the finest degree of refinement, one group of twins will seem to merge into the other. . . . . The data presented in this paper, and the theoretical conclusions derived from these data, tend to explain why we may expect some monozygotic twins to be hardly more similar than some few of the most similar dizygotic. Some of the latter, either because they are derived from parents genetically very similar, or from a chance repetition of much the same genetic complex in two zygotes formed simultaneously, may be as similar in most common features as are a few of the most different monozygotic twins." (*Ibid*, p. 215). Therefore, the really crucial point of Newman's entire argument is the validity of his statement: "But the latter will be identifiable as such through a study, especially in connection with dermatoglyphics, of the peculiar workings of the asymmetry mechanism heretofore described." (*Ibid*, p. 215).

Newman has interpreted the dermatoglyphics of my cosmobia as well as Wilder's. My cases T 60, T 61 and T 16 are regarded as excellent illustrations of partial bilateral symmetry. (See case T 61 above). But note that case T 144 is apparently an example of total bilaterial symmetry. Thus:

Right twin
(R) 8.6.5'.3./0.0.0.0.L.C12.
(L) 8.6.5.3./0.0.0.0.L.C12.
Left twin
(R) 7.5'.5.3./M.0.0.0.L.C13.
(L) 7.5".5.3./0.0.0.0.L.C12.

What evidence has Newman brought that such a condition cannot exist in fraternal twins? Is it not merely an assumption that the gross differences in these palms are the result of segregation of right and left handed characteristics? I cannot convince myself that dermatoglyphics has been placed by Newman on such a sure basis—sure because objective and quantitative—that we may now depend upon it for accurate results in the diagnosis of monozygotism.

In my paper (*Ibid* I, p. 166 and p. 167) I called attention to the similarities found by Waardenburg, Wilder and Lauterbach in the palm and finger prints of individuals who were obviously not monozygotic twins. Newman has unfortunately not commented on these reports although the publication of Waardenburg should have been of particular interest to him. This author found two brothers, the one eight years older than the other, whose fingers were absolutely alike in their fundamental features except for a small pattern on one finger which was not completed in the homologue of the brother. Would Newman have interpreted this as a case of partial segregation?

Cummins who is probably the greatest living authority on dermatoglyphics has made a careful study of the palm, plantar and finger prints of *unselected* twins and attempted a quantitative estimation of the differences he found. He was troubled with the same feeling of doubt which impressed me when I attempted dermatoglyphic diagnosis. His results did not concur with the conclusions reached by examination of the birth membranes. He attempted to apply Newman's dicta (also suggested by Komai) that "in monozygotic twins one palm of the twin resembles more closely one or both palms of the other twin than it resembles the other palm of the same twin." (NEWMAN—*ibid*, No. 7, p. 215). Cummins carefully measured the bilateral differences between the twins of a pair and concludes that "the seriation demonstrates that the twin varieties are not to be differentiated by the relative magnitudes of bilateral and intertwin differences" (CUMMINS—*Anatomical Record*, Vol. 46, p. 196, 1930).

Rife has recently studied 20 pairs of monozygotic twins and 100 pairs of sibs (RIFE—JOURNAL OF HEREDITY, 24:407). He finds that the measurement of differences in finger prints according to Bonnevie shows greater similarity between monozygotic twins than between sibs. He points out, however, that the statistical study masks individual instances in which there were no real differences between the finger prints of sibs and monozygotic twins. Mirror-imaging and the study of differences of right and left hands of one twin as compared with those of the same members of both twins also failed to aid Rife and hence he comes to the conclusion that "these data seem to indicate that the value of finger prints for determining monozygosity has been considerably exaggerated, although when used with other criteria, they are often good indicators." In the study of differences in iris pigmentation he finds a more suited measure and expresses great confidence in this method. It may be that the painstaking anthropological technique of Rife is in part responsible for his success, but this can hardly remove the difficulty of age change which appears at present inescapable when the method is applied to children. As far as is known to us, no one has proven that the changes in iris pigmentation occur simultaneously in monozygotic twins.

In a previous article Rife presented a plan for the diagnosis of monozygotic twins which to our mind is more promising than previous attempts, because it permits an estimation of the degree of similarity existing between two individuals supposed to be "identical" twins (RIFE—JOURNAL OF HEREDITY). The reader is referred to the original article for the details. Naturally only the application of Rife's method on many hundred twins will establish its practical value.

I am, therefore, still of the opinion that:

(1) The present methods of differentiating monozygotic from dizygotic twins are not sufficiently accurate to define the position of the individual case.

(2) Twins constitute a series; the extreme ends are palpably clear but the mid-group represents individuals of an uncertain character. Here subjective elements play a very dangerous rôle when diagnosis is attempted,

(3) Newman has not yet proved the validity of his dermatoglyphic methods.

I wish to express my appreciation to Professor T. Wingate Todd, Henry Willson Payne Professor of Anatomy, Western Reserve University, for encouragement and criticism although, of course, responsibility for any opinions expressed rests with me.

Brief Reply to Reichle's Article

H. H. Newman

Reichle seems to accept the principle that twins, from the standpoint of their origin, belong to two distinct categories, monozygotic and dizygotic, but he is skeptical about the possibility of diagnosing with accuracy all individual cases as belonging to one or the other category.

The difficulty in his case is, I believe, that he is interested mainly in diagnosing fetal or new-born twins. In such twins the resemblance method of diagnosis now used by practically all twinning specialists is of little value because of the lack of well defined hereditary resemblances and differences in such young individuals. He is forced, on this account, to resort to fetal membranes or to dermatoglyphics as his criteria for diagnosis. The question arises as to which of these two criteria is more reliable as a criterion of zygosity.

Curtius, Lassen and Verschuer, who have given a great deal of attention to the question, claim that a rather large number of pairs of twins of the most strikingly "identical" sort are clearly dichorial, though the majority of "identicals" are monochorial. All cases diagnosed by the similarity method as fraternals were dichorial, though some exhibited a false monochorial condition due to fusion. These discoveries, if well founded, do actually render the membrane method of diagnosis unreliable. The real point at issue is whether the German workers are to be relied upon. I have no grounds for questioning their findings.

The question then arises as to whether dermatoglyphics alone afford a satisfactory substitute for fetal membranes as a diagnostic criterion. Reichle seems to believe that I have espoused dermatoglyphics as an all sufficient criterion for diagnosing twins. No such view as this have I ever held. In actual practice, after years of experience, I now diagnose identical twins at a glance as though through a sixth sense. For the last three years during which I have examined scores of twins, I have had no more than a momentary hesitancy as to diagnosis. In every case this original diagnosis has been fully confirmed by detailed comparisons capped off by a study of palm and finger prints. Siemens and Verschuer indicate that they have had essentially the same experience. Thus the diagnosis of twins after they have reached an age of six or seven years is relatively simple, for doubtful cases are very rare.

Now the method just described is, I believe, objective enough in essence, for it depends on a trained sense of resemblances and differences that is probably more discriminating than one ob-

tained with instruments of measurement. But the resemblance method depends on the cumulative force of large numbers of resemblances, not upon any one or a few. Palm prints and finger prints constitute two very good tests of resemblance. Usually the diagnosis is adequate without referring to palm and finger prints, but the latter add the clinching evidence. The few cases where the dermatoglyphics do not at first blush support the resemblance diagnosis are usually due to reversals of asymmetry in some of the pattern elements. Unless one understands these reversals he may miss the actual resemblance that exists.

Reichle accuses me of overlooking as a possible cause of differences in identical twins anastomoses of blood vessels in the common placenta. As a matter of fact, a whole chapter in my *Physiology of Twinning* is devoted to that subject.

The basis for my disagreement with Reichle's statement that epidermal ridges appear only late in fetal life is the work done in this field by Bonnevie and by Cummins. Both of these authors agree that the pattern is fixed during the third month of fetal life. In one sense this is early, but in another sense it is late. There is no ground for controversy in this matter.

The editor has requested me to be brief in this reply. Hence it will not be possible to discuss all the issues raised. I appreciate the difficulty of Reichle's problem and would like to be of service in helping him in diagnosing some of his more baffling cases.

The Rule of Social Law

ANIMAL LIFE AND SOCIAL GROWTH, *By* WARDER CLYDE ALLEE. Pp. 159, price $1.00. The Century of Progress Series. The Williams and Wilkins Co., 1932.

THE balance of nature and all that it implies calls, for its interpretation, on every bit of available knowledge the biologist has and a good deal that he at present lacks. The average biological problem can be more or less isolated and analyzed independently of associated phenomena. This is not so easy when we are trying to determine the condition of social life and growth; of the organism in relation to a nature that has many moods. Here every factor is important in relation to every other one and, within limits, they must be studied together. But, difficult though these biological jigsaw puzzles are, we cannot pretend to have any adequate conception of our living universe and its bearing upon our lives unless the parts of these puzzles are pieced together at least sufficiently well to suggest what the completed design is going to look like. To pave the way for a more general understanding of "nature in the raw" as well as of nature tamed. Doctor Allee has written his little book of 159 pages.

Despite its obviously popular intentions one may judge from the statements on the jacket and in the preface that the material is intended for a reader whose interests are distinctly above those of the average "browser." On the jacket appears the comment that "the material herein is of as much interest to psychologist, sociologist, and student of human affairs generally as it is to the nature lover." And in the preface: "this small book is presented as a fairly simple statement of a series of interesting facts which belong together although they are not usually treated so." The book meets admirably the needs of those working in the indicated border line fields and of the more serious of the general readers. There is a wealth of well selected evidence in the ten chapters that should prove both helpful and stimulating to many.

I feel though that the very mass of "case histories" which makes the book helpful to the general biologist, psychologist, and sociologist may cloud the issue to a certain extent for the more general, however intelligent, reader who also has a right to some notion as to how the living world runs. Some day I hope that Doctor Allee will write a companion volume (and perhaps a thicker one) for this larger group. It would be futile for anyone lacking a first hand knowledge of the manifold problems involved in keeping nature on an even kneel to attempt a popularization of this story but this Doctor Allee, equipped both with the necessary background and with the sense for symplifying and enlivening of scientific data, could do well.

The chapter headings indicate the scope of the book. I. The Animal Community; II. Animal Habitats; III. Community Analysis; IV. The Organization of Land Communities; V. The Evolution of Animal Communities; VI. Unbalance in Nature; VII. Aggregations of Animals; VIII. Physiological Effects Produced by Aggregations; IX. Structural Effects Produced by Aggregations; X. The Higher Social Levels.

One wonders what the reaction of the rulers of Chicago or of Pittsburgh or of any other large and modern city might be to the book's concluding paragraph:

> Social organizations in which laws, not individuals, rule are the highest social development. In human societies, we say that there is a respect for law and order. In bee or ant societies, we say that they are controlled by "the spirit of the hive." Such social organizations have grown a long way from that existing in ecological communities where the struggle between different members is much more easily found than is evidence of their underlying co-operation. The highest organizations even tend to dispense with leadership; the individuals composing the group become entirely group centered rather than individually minded. There is neither individual authority nor obedience, for neither is needed in the face of complete co-operation for the common good.

ROBERT T. HANCE.
University of Pittsburgh.

Books Received

BOOKS are acknowledged in this column as received, and such acknowledgment must be regarded as sufficient return for the courtesy of the sender. As far as space permits, books that contain material of special interest to the readers of the JOURNAL *will be reviewed in later numbers.*

HUMAN PROBLEMS OF AN INDUSTRIAL CIVILIZATION, *by* ELTON MAYO, Professor of Industrial Research, Graduate School of Business Administration, Harvard University. Pages, 194, index. Price $2.00. The Macmillan Company, New York, 1933.

The title of this book is somewhat misleading because what is meant is *personnel* problems in an industrial civilization. Human problems include population, its quantity and quality,—which important matters are not dealt with in this book.

HEREDITY AND ENVIRONMENT, Studies in the Genesis of Psychological Characteristics, *by* GLADYS C. SCHWESINGER, Edited by Frederick Osborn. Pages, 484, indices, appendix. Price $4.00. The Macmillan Company, New York, 1933.

A very commendable attempt to evaluate existing tests of intelligence and personality, and the effect of environment and genes in making the man.

NATURE AND NURTURE, *by* LANCELOT HOGBEN, Professor of Social Biology in the University of London. Pages, 143, index and appendix. Price $2.75. W. W. Norton & Co., New York, 1933.

A plea for a foundation of *actuality* on which to erect test structures in studying the effects of genes and of the external world. The approach is very different from that of Schwesinger.

GROWTH, *by* the late JAMES LORRAIN SMITH, M. D., LL.D., F. R. S., Professor of Pathology, University of Edinburgh.

Edited by J. S. Haldane, C. H., M. D., F. R. S. Pages, 135. Price 6/ net. Oliver and Boyd, London, 1932.

One, perhaps *the*, fundamental peculiarity of living things is contained in the mystery of why they grow. That they do transcends most existing theories, mechanistic and vitalistic, toward which latter concept Lorrain Smith appears to lean.

TRAITÉ DE MÉDÉCINE DES INFANTS, Tome I, published under the direction of P. NOBECOURT and L. BABONNEIX. Pages, 884, illustrations and graphs. Masson et Cie, Paris, 1934.

Including those traits passed on by defective heredity.

PRACTICAL PEDODONTIA OR JUVENILE OPERATIVE DENTISTRY AND PUBLIC HEALTH DENTISTRY, An Introductory Text for Students and Practitioners of Dentistry, *by* FLOYDE EDDY HOGEBOOM, D. D. S., Professor of Children's Dentistry, University of Southern California. Special Chapters by authorities in the various fields of dentistry. Third edition, 328 pages, index, illustrations. The C. V. Mosby Company, St. Louis, 1933.

In spite of a chapter on endocrinology only a single reference to heredity (and that in another chapter) is to be noted. This is more remarkable since dentists are constantly brought into contact with inherited anomalies of the teeth.

MANUAL OF THE SOUTHEASTERN FLORA, being descriptions of the seed plants growing naturally in Florida, Alabama, Mississippi, Eastern Louisiana, Tennessee, North Carolina, South Carolina and Georgia, *by* JOHN KUNKEL SMALL. Pages, 1554, illustrations, index. Published by the author, New York, 1933.

Nature's handiwork in diversification and adaptation over a large region, in a form of great value to the specialist but hardly palatable to the lay reader.

THE CONQUEST OF A CONTINENT or the Expansion of Races in America, *by* MADISON GRANT, President, New York Zoological Society; Trustee, American Museum of Natural History; President, Boone and Crockett Club; Councillor, American Geographical Society. Introduction by Prof. Henry Fairfield Osborn. Pages, 393, bibliography, index, and 14 maps. Price $3.00. Charles Scribner's Sons, New York, 1933.

The "Passing of the Great Race" into a second volume.

AMERICA SELF-CONTAINED, *by* SAMUEL CROWTHER. Pages, 340. Doubleday, Doran & Company, Inc., New York, 1933.

An important step in making something of ourselves racially is to stand where we are and dig in and make our civilization a self-sufficient, going concern. Crowther councils the same action on grounds of economic expediency.

▼

Further Light on the Origin of the Marsh Seedless Grapefruit

SINCE the account of the origin of this variety appeared in the *Journal*, the writer has had an opportunity, through the courtesy of Professor H. H. Hume of the Florida Agricultural Experiment Station, to examine the *Catalogue of the Lakeland Nurseries* for the season of 1896-97, in which C. M. Marsh, the proprietor, made the first announcement of the seedless variety of grapefruit. It was catalogued as the "Marsh Seedless Pomelo (Grapefruit)." While the announcement gave no details as to its place of origin it states that "The old original seedling tree from which we have our propagation was a prolific bearer for 30 or 40 years and a three-year-old bud will bear all the fruits that it should hold at that age."

T. R. ROBINSON.

A HEREDITARY LETHAL MUSCLE CONTRACTURE IN CATTLE

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SOME time ago Dr. J. E. Thompson, city chemist and veterinarian of Ely, Minnesota, reported to the writer a peculiar set of circumstances encountered in his veterinary practice during 1931. Upon subsequent investigation the condition responsible for them has proved to be one of considerable interest to geneticists, veterinarians and breeders of live stock.

In three herds of Holstein-Friesian cattle which had been under his observation for many years, and which during that time had been practically free from even the simplest forms of dystocia, there occurred in one year five cases in which the calves were alive at full term, but were afflicted with an abnormality so extreme that only two could be delivered entire and then only with great difficulty. To deliver the other three it was necessary to dismember them in the uterus.

All of these five calves were sired by the same bull. They were so much alike that, after encountering two of them in the *A* herd, Dr. Thompson was able to diagnose the condition at once upon manual examination of two cases of difficult parturition in two other herds and to predict before being told that the afflicted calves had been sired by Mr. *A*'s bull.

None of these calves were seen by the writer of this article but the description given by Dr. Thompson is adequate to permit ready identification of the condition. The affected calves apparently caused distress to their dams before the end of the gestation period but were alive at full term. The head was drawn up toward the back, apparently by contraction of the cervical muscles, and the neck was extremely rigid. The fore and hind limbs were folded and almost wrapped around the body. Like the neck, they were characterized by extreme rigidity, there being very little movement, if any, at the joints. All five calves were the same. The sexes were not noted.

Allegedly an Acquired Character

A peculiar coincidence was that in the year before these calves were encountered their sire had been injured by getting a chain embedded in his neck. After the wound healed the neck was stiff for a long time. The owner stated that when the bull was taken out for service the painful condition and stiffness in the neck seemed even more pronounced than at other times. When this situation was followed by the appearance among his offspring of five calves exhibiting among other complications extreme rigidity of the neck, it was not unnatural that the owner should ascribe the abnormality of the calves to the injury of the sire. This interpretation seemed all the more likely because, although the bull was nearly six years old, his progeny had all been normal prior to the injury to his neck, which had occurred in his fifth year.

Disregarding the Lamarckian aspect of the case, it seemed fairly certain that the abnormality was a hereditary muscle contracture similar to that found by Roberts[5] in lambs. The possibility that it might have been occasioned by nutritional disorders seemed unlikely because of its occurrence in three separate herds. Furthermore, while one might expect occasional teratological conditions in any herd, it was extremely unlikely that five cases so much alike should

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appear in the progeny of one sire unless the condition were hereditary. Moreover, the abnormality does not present any of the usual teratological conditions. Dr. Thompson stated that he had never encountered anything like it in many years of practice until these five cases occurred, all in one year.

Accordingly the writer visited the owners of the three herds in company with Dr. Thompson, and, thanks entirely to the latter's proficiency with the Finnish language, managed to trace the relationships of the cattle concerned.

Genetic Evidence

The sire of these calves had been born at the *A* farm. Though both his sire and his dam had been purebreds and registered, this bull had never been registered. For convenience he will be designated as Bull *X*.

The five cows producing the abnormal calves were as follows:

Herd A

No. 1. Daughter of Bull *X*.

No. 2. Daughter of Bull *X*.

No. 3. Half-sister to Bull *X*, by the same sire but from a different dam.

Herd B

No. 4. Daughter of Bull *X*.

Herd C

No. 5. The descent of this cow could not be definitely traced but it was found that a number of the *C* cows had been sired by bulls in the *A* herd. It was therefore quite probable that No. 5 had been sired either by Bull *X*, or by *X*'s sire. It will be shown later that she might have gotten the gene or genes for the muscle contractures from either of these bulls.

It is obvious from these facts that the condition is hereditary. Its appearance late in the breeding life of *X* was in no way related to the injury to that bull's neck, but rather wholly the result of his then being mated to his own daughters, some of whom carried the same gene or genes for muscle contractures as he did. By chance three of them produced calves homozygous for the abnormality.

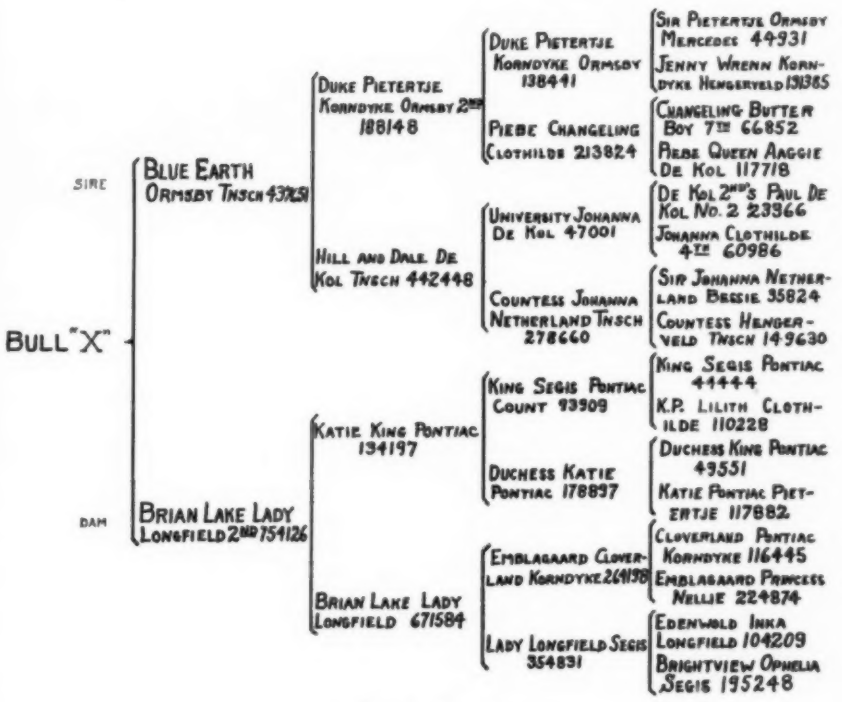
Owing to incompleteness of the available records, no attempt was made to determine the ratio of affected calves to normal ones, or the proportion of carriers of the gene to non-carriers among the progeny and half sisters of Bull *X*. The bull was slaughtered after these calves appeared, so that further breeding trials were impossible. There can be little doubt, however, that the muscle contracture is a simple recessive character. If it were dominant it would immediately be eliminated from the germ plasm. If it were dependent upon duplicate recessive genes, or were inherited in any other way, it is extremely unlikely that three daughters out of a very limited number should have produced it in one season.

MUSCLE CONTRACTURES FATAL

Figure 13

Calf born at full term with contractures of the neck and limbs. This condition is not only fatal to the calf, but in some instances to the dam as well. The photograph shown above is of a calf born in Norway, and is reproduced through the courtesy of Dr. Otto Mohr. Similar lethal abnormalities have been reported in sheep and swine.

Cow No. 3 had given birth to four normal calves sired by Bull *X*, prior to the one with the muscle contractures. Such a ratio is strongly suggestive of a simple recessive character although the data are limited. Moreover, the available evidence that the abnormality is a unifactorial recessive character is supported by the fact that a condition very similar to that described in this paper, if not identical with it, has been shown to behave as a simple recessive charac-


BULL "X"
SIRE
BLUE EARTH ORMSBY TNSCH 43729
DAM
BRIAN LAKE LADY LONGFIELD 2ND 754126
DUKE PIETERTJE KORNDYKE ORMSBY 2ND 188148
HILL AND DALE DE KOL TNSCH 442448
KATIE KING PONTIAC 134197
BRIAN LAKE LADY LONGFIELD 671584
DUKE PIETERTJE KORNDYKE ORMSBY 138441
PIEBE CHANGELING CLOTHILDE 213824
UNIVERSITY JOHANNA DE KOL 47001
COUNTESS JOHANNA NETHERLAND TNSCH 278660
KING SEGIS PONTIAC COUNT 93909
DUCHESS KATIE PONTIAC 178837
EMBLAGAARD CLOVERLAND KORNDYKE 264198
LADY LONGFIELD SEGIS 354831
SIR PIETERTJE ORMSBY MERCEDES 44931
JENNY WRENN KORNDYKE HENGERVELD 191385
CHANGELING BUTTER BOY 7TH 66852
PIEBE QUEEN AAGGIE DE KOL 117718
DE KOL 2ND'S PAUL DE KOL NO. 2 23366
JOHANNA CLOTHILDE 4TH 60986
SIR JOHANNA NETHERLAND BESSIE 35824
COUNTESS HENGERVELD TNSCH 149630
KING SEGIS PONTIAC 44444
K.P. LILITH CLOTHILDE 110228
DUCHESS KING PONTIAC 49551
KATIE PONTIAC PIETERTJE 117882
CLOVERLAND PONTIAC KORNDYKE 116445
EMBLAGAARD PRINCESS NELLIE 224874
EDENWOLD INKA LONGFIELD 104209
BRIGHTVIEW OPHELIA SEGIS 195248

PEDIGREE OF BULL *X*

Figure 14

Pedigree of the bull producing the lethal muscle contracture. The evidence indicates that he inherited the gene for the lethal character from his sire, Blue Earth Ormsby Tnsch.

ter in two other species of the same sub-order of mammals.

Effects of the Abnormality

The abnormality is not a character that lends itself to the usual genetic analyses based on the ratio of affected calves to normal ones in the progenies of various types of matings. The homozygous recessive condition is not only lethal to the calf but, in some cases, also to the dam. Of the five females producing calves with contractures, Numbers 1 and 2 were heifers carrying their first calves. Both of the latter had to be dismembered to overcome dystocia. In spite of every care, one heifer was fatally injured and the other suffered such irreparable damage that she could not subsequently be gotten in calf. Cow No. 3 was apparently unaffected but the later history of the other two is not known. It seems probable that the birth of a calf with contractures is fraught with grave risk for all the dams and that that risk may be accentuated for primiparae.

The facts that birth of a homozygote may eliminate a female heterozygote from further breeding tests, and that the chance of such elimination occurring may be less if the heterozygote has already produced one or more normal calves, would not be conducive to the securing of 3:1 and 7:1 ratios unless more females were tested than is ordinarily possible with

cattle. The alternative method is to determine the proportion of animals carrying the gene in two different kinds of populations. These, and the proportion of carriers expected in each if the character be a simple autosomal recessive, are as follows:

Type of Mating	Heterozygotes expected among living progeny
(1) Heterozygote x normal	one-half
(2) Heterozygote x Heterozygote	two-thirds

Pedigree of Bull *X*

In view of the possibility that the congenital lethal muscle contracture may be encountered in cattle elsewhere, and to point out lines of descent in which it may be expected to occur, the pedigree of Bull *X* is shown for four generations. (Figure 14.)

There is some evidence that the gene for contractures has descended in the sire's side of the pedigree, rather than in the dam's side. Bull *X* had been mated to two half-sisters in the *A* herd, one from the same sire as *X*, the other from the same dam. The owner stated that the latter cow had produced five normal calves by Bull *X*. The other was Cow No. 3, who had produced by *X* four normal calves and one with the abnormality. It would seem therefore that both Bull *X* and Cow No. 3 had inherited a gene for the lethal muscle contractures from their common sire, Blue Earth Ormsby Tnsch.

There is no way of knowing at present whether the mutation had arisen in this animal or whether he had inherited it from his parents. It is hoped that the publication of this note may lead to recognition of the condition if it occurs elsewhere, and that further studies of the pedigrees concerned will reveal the common ancestor responsible for the dissemination of the gene.

Geographical Distribution of the Gene

After most of this investigation had been completed the writer learned that a condition very similar to the muscle contracture described in this paper, and probably identical with it, had been reported and illustrated by Mohr.[4] Although his communication gives few details of the condition, a comparison of notes with Professor Mohr at the Sixth Genetics Congress made it fairly certain that the character found in northern Minnesota was identical with that discovered by him near Bergen, Norway. By courtesy of Professor Mohr, one of the two such specimens reported there up to August, 1932, is shown in Figure 13.

Dr. Thompson states that Professor Mohr's specimen resembles his except that in the case at Ely the head was not twisted to one side but was drawn up toward the back.

In his report of a similar condition in the pig, Hallquist[2] briefly states that he and Dr. Bonnier have found in a race of Swedish cattle a condition which is apparently similar to that described in this paper if not identical with it.

If the abnormality had occurred in man instead of in cattle some connection might be suspected between its occurrence in Minnesota and in the Scandinavian countries. It seems more probable that the mutation has arisen independently in the American Holstein-Friesians than that it has been distributed to Norway, Sweden and America from some single source, but the latter possibility cannot be ruled out.

A hereditary condition somewhat resembling that discussed in this paper has been found in Red Danish cattle by Loje.[3] The affected foetuses have the same rigidity of the limbs, but differ in having short necks and in being dead and partially mummified at birth. Death apparently occurs about the eighth month of gestation. The differences between this abnormality and that found in Minnesota may mean that two different genes are producing characters some-

what alike, or may possibly result from different modifying factors in the two breeds interacting with a single gene for contractures.

It is entirely probable that the abnormality has occurred elsewhere in this country, but has not been recognized as being hereditary. To his description of 16 teratological specimens Williams[6] adds accounts (Case 17) of two calves characterized by extreme rigidity of the body and legs. One of these was alive at full term and had to be dismembered before delivery. It had the same upward bend of the neck as had Dr. Thompson's specimens. It is not improbable that these calves were also afflicted with hereditary contractures, but that, in the absence of inbreeding, not enough abnormal ones were produced to permit recognition of the hereditary nature of the condition.

Comparative Genetics

It is of special interest that hereditary lethal contraction of the limbs has also been found in two other species of the Artiodactyla. Roberts[5] has conclusively proven it to be a simple recessive condition in sheep, and Hallquist[2] has made extensive studies of it in swine with the same results. In swine the symptoms would appear to be somewhat less extreme than those in cattle and sheep. The hind limbs are not commonly affected, and Hallquist does not mention rigidity of the neck. In three of Roberts's affected lambs examined in detail by Mr. Wm. C. Miller, joints of both fore and hind limbs were affected. Two of the three had wryneck. The contractures in sheep would appear to resemble those in cattle more closely than does Hallquist's "krummsteifbeinige" condition in swine. This is to be expected.

From the standpoint of comparative genetics it would be of interest to learn if this abnormality ever occurs in other species of the Artiodactyla or in species outside of that sub-order. It is not impossible that it may yet be recognized in some of the army of laboratory rodents or in man.

Discussion

The lethal condition described in this paper is one of a considerable number of fatal abnormalities of domestic animals which have been shown to be hereditary. Thirty of these are listed in a recent review.[7]

At least eleven such characters are now known in cattle. It is remarkable, however, that apart from the occurrence of bull-dog calves in the few imported Dexter cattle, only one other hereditary lethal character has been found in domesticated mammals in North America. That one is the epithelial defect studied in calves by Hadley and Cole.[1]

One reason why these lethal characters have been found more frequently elsewhere is probably that in some of the smaller countries of Europe there has been somewhat more inbreeding of stock than has been practised in this country. This is particularly likely to occur in some of the geographically isolated districts in the Scandinavian countries where free interchange of stock is less common and where several lethals have been found. Such a condition was responsible for the discovery of the condition at Ely, Minnesota. The *A* herd had one of the few purebred Holstein bulls in a comparatively isolated district and accordingly the bull was utilized for several years by the three herds in which the abnormality occurred. The resultant inbreeding brought the hereditary contractures to light.

It is even more probable, however, that the discovery of so many lethal factors in Europe is the direct result of the researches of such investigators as Professor Otto Mohr and the late Christian Wriedt, and of the general interest stimulated by their findings. There can be little doubt that many lethal factors are interfering with the reproductive efficiency of domestic animals the world over. It is to be hoped that breeders of live stock and veterinarians will become

familiar with the hereditary lethal characters now known and thus be in a position to expedite the investigation of hitherto unrecognized lethals as they occur.

The only method of eliminating these characters is to refrain from breeding animals known to be carriers of a lethal gene. The methods for detecting these carriers are familiar to all geneticists and their applications have been adequately discussed elsewhere.

Congenital muscle contractures would appear to be of somewhat greater economic importance than the average lethal character because of the extreme dystocia which must be overcome before the calf can be delivered. The inevitable loss of the calf may not be considered serious in some dairy herds but the associated risks of fatality and sterility for the dam make the character a doubly injurious one.

The data presented in this paper are not as extensive as could be desired, but there seems no probability of amplifying them in the near future because the slaughter of Bull *X* left no known male carrier of the factor for contractures. One of the carrier cows was fatally injured; another was marketed because of sterility and at least two others have also been disposed of. Accordingly the available data are published in the hope that this account may lead to the recognition of the condition and the accumulation of further data if the character appears elsewhere.

Summary

Five calves sired by one Holstein-Friesian bull in Minnesota were afflicted with extreme rigidity of the neck and of all their limbs. The long bones of the latter were bent and in some cases the limbs seemed almost wrapped around the body. The condition is considered to be caused by extreme contractures of the muscles of the neck and limbs.

All five were alive at full term, but died during or after delivery. Three had to be dismembered to overcome dystocia.

One heifer was fatally injured during delivery of her affected calf and another was rendered permanently sterile.

Three of the five cows producing calves with contractures were daughters of the sire and another was a half-sister. Other evidence is presented which indicates that the muscle contracture is a simple recessive lethal character. An extended pedigree of the sire of the five calves is given and the gene is traced to the bull Blue Earth Ormsby Tnsch. Numbered 437651 in the American Holstein-Friesian herd books.

The occurrence of similar or identical abnormalities in Norway, Sweden, and the State of New York is briefly discussed. It is pointed out that very similar hereditary abnormalities occur in sheep and swine.

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A Glossary Of Genetic Terms

GENETICS has developed so rapidly that many terms in common use in genetic literature are not yet incorporated in the dictionaries. Many people who are interested in heredity are bothered by such words, for which no definition can conveniently be found. For this reason, it is proposed to publish occasionally a glossary of terms which would not be readily understood and which are not to be found except in new unabridged dictionaries. It is realized that these definitions will probably not meet with universal approval and comments and suggestions will be welcomed.

AGGLUTINOGEN—A substance from which an agglutinin is produced; which, in turn, is a substance which has the property of clumping and precipitating certain specific types of blood corpuscles or bacteria.

ALLELOMORPH—Mendelian characters are inherited in alternative pairs, or in alternative series. These alternative genes are called allelomorphs. Examples: Albinism (recessive), Normal pigmentation (dominant); Horns (recessive), Hornlessness (dominant).

BACKCROSS—Cross of a hybrid to one of the parental forms.

CHROMOSOMES—Tiny dark-staining bodies visible in the nucleus of the cell at the time of cell-division. The number of chromosomes in any species is usually constant. The chromosomes carry the genes, linearly arranged, which control the development of Mendelian characters.

COSMOBIA—Conjoined twins.

DIZYGOTIC—Originating from two fertilized eggs. Dizygotic twins are genetically no more similar than ordinary brothers and sisters.

DOMINANT—A character possessed by one of the parents of a hybrid, which appears in the hybrid to the exclusion of the contrasted character from the other parent (the recessive). Thus in a cross of green- and yellow-seeded peas the first generation has yellow seeds. Yellow is dominant and green is recessive, being transmitted but not appearing in the presence of the factor for yellow.

DYSTOCIA—Difficult birth.

FACTOR—Same as *Gene.*

F_1—(Pronounced eff-one). The first filial generation. The offspring of a given mating.

GAMETE—A reproductive cell of either sex; e. g. sperm or ovum.

GENE—(1) The unit of inheritance, which is transmitted in the germ cells, and which by interaction with the genic and cytoplasmic complex controls the development of a character. The genes are arranged linearly in the chromosomes. (2) "The physical basis of heredity"—or more figuratively, the atom of the genetic molecule.

HETEROZYGOUS—Containing both genes of an allelomorphic pair, or two genes of an allelomorphic series. Heterozygous individuals generally resemble the individuals homozygous for the dominant character, but they transmit the recessive to half their offspring.

HOMOZYGOUS—(1) An organism formed by like germ cells; (2) An organism is said to be homozygous for a given character when all the germ cells transmit identical genes for this character. This is the most frequently used meaning of the term.

LINKAGE—Association of genes in inheritance, due to the fact that they are in the same chromosome.

METAXENIA—Direct effect of the pollen on the parts of the seed and fruit lying outside the embryo and endosperm.

MONOZYGOTIC—Originating from a single fertilized egg cell, which in the course of subsequent development has given rise (by fission) to two or more individuals. Twins (or triplets, etc.) of such origin are, barring mutations, genetically identical.

MULTIPLE FACTORS—In the expression of some characters more than two genes are necessary. Such cumulative or cooperative genes are called multiple factors.

PARTHENOCARPY—Production of normally formed fruit with no seed or with empty seed. Two kinds are recognized by Winkler: stimulative, through agency of pollination, insect injury or other stimulative action; vegetative—wherein a sterile fruit develops without any other stimulation as in the navel orange.

RECESSIVE—See *Dominant.*

SEGREGATION—Separation of allelomorphic genes at meiosis.

SELFED—Self-pollinated.

SYNDROME—A group of symptoms occurring together.

ZYGOTE—An organism produced by the union of two germ cells.

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